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THE PSYCHOLOGICAL REVIEW

ORGANISMIC ACHIEVEMENT AND ENVIRONMENTAL PROBABILITY[1]

BY EGON BRUNSWIK

University of California

I

The term 'achievement' will be used here as a generic term for the relationships better than chance existing between, and due to, an organism and variables in its physical environment. By physical environment we mean the 'geographical' surroundings as well as the stages along the 'historic' axis of the organism, that is to say, its past and future. Relatively stable relationships between organism and environment are among the descriptive features of the patterns found in observing life and behavior. Thus the recognition of such functional units need not, and will not, involve us in explanatory problems such as the alternative of mechanism vs. teleology.

Organismic achievement may extend in two main directions; (1) specificity regarding certain stimulus variables as antecedents or causes of reactions, and (2) specificity regarding certain results of organismic reaction. Examples for the former may be taken from the field of perception, for the latter, from the field of overt behavioral effects.

In the establishment of any kind of achievement one may distinguish two phases: (1) the portions of the causal chains

[1] This article, and the two following articles by C. L. Hull and K. Lewin, respectively, were addresses given at the Symposium on Psychology and Scientific Method held as part of the Sixth International Congress for the Unity of Science, University of Chicago, September, 1941.

Some of the author's remarks in the discussion concerning the papers of Hull and Lewin, as well as some of the replies to criticisms of the present paper, appear as footnotes here. Some minor changes have been made in the paper itself and a list of references has been added.

within the physical environment, and (2) the portions of the causal chains within the organism. We will discuss the environmental portions first. In each case, variables not located at (that is to say, not defined in terms of) the boundary between organism and environment will be used as examples in order to make explicit the characteristic entanglements within the causal texture of the regions in question. As far as the environment is concerned, one will have to start from the so-called 'distal' stimulus- or effect-variables as reference points and study their relationships to the proximal (or boundary-) variables.

On the perception side, an example of a distal variable is the distance of objects. Causal chains determined by distance will, on their way into the organism, exert certain proximal effects, or criteria, upon the sensory surface of the organism. The most important feature of the general relationship between distal and proximal stimulus variables is its lack of univocality.

Firstly, there is ambiguity in the direction from cause to effect. Inventories of possible 'cues for third-dimensional distance' have been compiled from the beginnings of psychological inquiry. Current textbooks list something like ten depth criteria, such as binocular parallax, convergence of the eye axes, accommodation, linear and angular perspective, interception of far objects by near objects, atmospheric effects, number of in-between objects, vertical position. The list could be extended considerably further. The necessity for becoming so involved derives from the fact that none of these proximal variables can be considered to be *the* distance cue in the sense of an effect which would be present without exception whenever the distal condition should obtain. Some of the cues will more often, others less often, be present, depending on circumstances, and occasionally all of them may be cut off (so that the fact of a certain distance relationship must remain unrecognized by the organism in question).

Secondly, there also is ambiguity in the reverse direction, that is to say, from effect to cause. A certain proximal stimulus feature, such as binocular parallax, may ordinarily be

due to differences in depth, but it could occasionally as well be caused by an artificial setup of two flat pictures in a stereoscope. Or, the characteristic trapezoidal shape of retinal images constituting the depth criterion of perspective may frequently be due to distortion of rectangular objects seen under an angle, *i.e.*, extending into the third dimension; but it may also be due to an actual trapezoidal object in a frontal position with all of its points at the same distance from the eye.

On the environmental portion of the effect side, the relationships between objects and cues are replaced, in a symmetrical fashion, by the relationships between means and ends, or between proximal actions or habits and distal results. Examples showing the ambiguity of these relationships in both causal directions could easily be given in analogous fashion to the ones discussed above for the perception side.

Generally speaking, both the object-cue and the means-end relationship are relations between probable partial causes and probable partial effects. Thereby the entire universe of the living conditions of the organism or species in question might well be taken as a 'reference class' defining a 'population' of situations. Then there is a good chance that distance as a distal variable will cause distance cues, and that the so-called distance cues have been actually caused by distance, but it will by no means be necessarily so. We use the term 'partial' (cause or effect) since the members of the causal chain are in every particular instance determined by a large number of other relevant conditions. Cues as well as means can be ranked into 'hierarchies' in accordance with the degree of probability by which they are linked, in both causal directions, to the respective distal variables, and classified accordingly as 'good,' 'misleading,' etc. (13).

This brings us to the second point, the contributions of the organism. Survival and its sub-units, which may be defined as the establishment of stable interrelationships with the environment, are possible only if the organism is able to establish compensatory balance in the face of comparative chaos within the physical environment. Ambiguity of cues and means relative to the vitally relevant objects and results must

find its counterpart in an ambiguity and flexibility of the proximal-peripheral mediating processes in the organism. This pattern contrasts somewhat with the relatively specific focussing of vital processes upon the central-organismic and the distal-environmental variables. Thus each class of behavioral achievement may be represented, when telescoped into a composite picture covering extended periods of time, by a bundle of light rays passing through a convex lens from one focus to another, with a scattering of the causal chains in the mediating layers. Most objective psychologists who have made efforts to find a formal criterion by which behavior could be delimited from non-behavior have in various forms resorted to something amounting to such a lens analogy. Examples are Tolman's "persistence and docility of activity relative to some end" (12) as a criterion for purposiveness of behavior, Hunter's emphasis upon 'vicarious functioning' in defining the subject matter of a behavioristic psychology, Hull's emphasis upon such patterns as the 'habit family hierarchy' (6), some considerations put forward by Heider (5), and generally the concept of 'equivalence' or of 'equipotentiality' of stimuli and of acts.

The point I should like to emphasize especially in this connection is the necessary imperfection, inflicted upon achievements—as relations between classes—by the ambiguity in the causal texture of the environment, which remains apparent as long as single variables, that is partial causes and partial effects, are considered under otherwise not specifically controlled conditions. Because of this environmental ambiguity, no matter how smoothly the organismic instruments and mechanisms may function, relationships cannot be foolproof, at least as far as those connecting with the vitally relevant more remote distal regions of the environment are concerned. This intrinsic lack of perfection, that is of univocality, will on the whole be the greater the more wide-spanning the relationships involved are. The only way in which perfection could be secured would be by control over all the remaining conditions which could possibly become relevant in the given case. This however is something the reacting organism cannot do for

lack of time if not for other more serious reasons—and thus something which the psychologist who wishes to catch and rationally to reconstruct organismic adjustment at large, with all of its faults and fallacies, should also not do. All a finite, sub-divine individual can do when acting is—to use a term of Reichenbach (11)—to make a posit, or wager. The best he can do is to compromise between cues so that his posit approaches the 'best bet' on the basis of all the probabilities, or past relative frequencies, of relevant interrelationships lumped together.

One of the comparatively neglected tasks of a molar environmental psychology is to find out the extent to which environmental hierarchies of probabilities of object-cue as well as of means-end relationships do find a counterpart in similar hierarchies of evaluation by the organism. This would mean that the environmental probabilities be first ascertained for all of the cues or means involved, with, say, the 'normal' life conditions of the organism taken as the defining reference class. This part of the research would be strictly environmental and preparatory in character and would not involve any reference to organismic reaction.[2] Very little has thus far been done in the direction of such an environmental analysis.

The most conspicuous exception is a certain knowledge we have about the so called 'physiognomic' relationships between certain mental states or abilities in our fellow-men, and their external physical characteristics. Such studies have, however, been undertaken primarily because of an interest centering in questions of the expressiveness of human beings viewed as subjects rather than because of an interest in some other subject's social environment and the problems confronting such a subject in his approach to objects of social perception. In effect, however, they have given us some information to be utilized for our purpose. The present writer has selected one of the few ascertained relationships found between ability and physique, namely the correlation of intelligence with height

[2] In the sense defined in footnote 3, such a statistical analysis of intra-environmental correlations would be termed 'psychological ecology' whereas the organism's proper adjustment to such correlations, to be expressed in terms of achievement, would be 'ecological psychology.'

and with weight. To be sure, these correlations are extremely low, about .15, but this is all to the good, since many of the cues probably in use in perception are of such a low order of validity, including some of the lesser members of what may be called, in analogy to Hull's habit-family-hierarchy, the 'cue-family-hierarchy' of the distance criteria listed on page 256. Social perceptual reactions to schematized drawings of human figures as well as to photographs which had been magnified, reduced, and distorted in height and width turned out to be more favorable with respect to apparent intelligence and other apparent personality characteristics in the case of taller and broader body builds (**2**, **14**). There thus may be intuitive responsiveness to social environmental correlations as low as .15 (though other possibilities of an explanation of the reactions, such as, *e.g.*, a psychoanalytic one, would have yet to be tested). Furthermore, there are indications that height and weight contribute only little to the impressions made, when compared with other factors such as the face, etc. Such a finding, if verified, would be in line with what should be expected on the ground of a perceptual compromise principle, since cues of low validity would then have to be given little weight by the organism in establishing the best bet.

The writer is attempting an analysis of the environmental validities of the distance cues, present, absent, and misleading or contradictory, in a set of pictures selected from magazines by a group of subjects and thus probably fairly representative of interesting life situations. The hierarchy thus established in a preliminary way appeared to be on the whole in fair agreement with what can be inferred from results of experimental studies about the subjective weight of distance cues which had been made to conflict artificially in stimulus configurations presented through the stereoscope.

On the whole, only scattered recognition has been given to the fact that object-cue and means-end relationships do not hold with the certainty obtained in the nomothetic study of the so-called laws of nature, but are rather of the character of probability relationships. This deficiency is most clearly reflected in the psychology of learning which has proceeded

almost exclusively along a dialectically dichotomized all-or-none pattern of 'correct vs. incorrect,' 'right vs. wrong.' Situations in which food can be found always to the right and never to the left, or always behind a black door and never behind a white one, are not representative of the structure of the environment, but are based on an idealized black-white dramatization of the world, somewhat in a Hollywood style. They are thus not sound as experimental devices from the standpoint of a psychology which wishes to learn, above all other things, something about behavior under conditions representative of actual life. In an effort to imitate experimentally the tangled causal texture of the environment more closely than is customary, the writer tested a variety of ambiguous environmental means-end relationships, using rats as subjects (1). The rate of learning (which may be taken as an index of organismic weight given to the means or cue in question) was found to vary with the probability, that is to say, with the combination of relative frequencies, of the intra-environmental relationships tested.

II

I have expanded on this subject to such an extent because I believe that the probability character of the causal (partial cause-and-effect) relationships in the environment calls for a fundamental, all-inclusive shift in our methodological ideology regarding psychology. To be sure, in the field of wide-spanning relationships of a predominantly historic-genetic type, such as of heredity and of individual differences in general, this shift occurred at the time of Galton and his followers who established correlation statistics as a particularly suitable means of quantitatively expressing ambiguous probability relationships. The relationships existing between organism and geographic environment at large will have to be approached in basically the same way. In any wide-spanning correlation, be it of historic or of geographic reference, there are a great many relevant variables and specific control is lacking for all of them except the two (or few), whose relationship is under specific consideration. Such a deliberate neglect of specific control of

relevant variables is the most fundamental negative characteristic of the 'molar' approach. Not more than generalized control by which membership is established in a broader class (including care for proper sampling) is exerted over the remaining relevant variables. For example, in comparing parents' intelligence with children's intelligence, not more than the most general features of the upbringing of the children such as health, normality, etc., are taken into consideration, instrumental detail is neglected. The situation is quite similar, though not quite so drastic, when we become interested in how well, in a practical achievement sense, we can estimate distal variables such as distance, or sizes and physical colors of objects, under all the varying circumstances of distance, surroundings, illumination, etc. (perceptual constancies). What the experimentalist is used to calling 'isolation of a variable' is in all these cases incomplete to a quite shocking extent. No univocality, no relationship resembling a 'law' in the traditional strict sense of the word can be uncovered under such circumstances.

The present paper thus represents an attempt to show that psychology, as long as it wishes to deal with the vitally relevant molar aspects of adjustment and achievement, has to become statistical throughout, instead of being statistical where it seems hopeless to be otherwise, and cherishing the nomothetic ideals of traditional experimental psychology as far as relationships between geographic stimulus variables and response variables are concerned. The price which has to be paid for such a double standard is the limitation of stimulus-response psychology to narrow-spanning problems of artificially isolated proximal or peripheral technicalities of mediation which are not representative of the larger patterns of life.

In particular, the extension of the principles of such an instrument as correlation statistics from individual differences to stimulus-response relationships involves, firstly, that instead of correlating two variables (*e.g.*, different test performances), paired by being drawn from the same sample of individuals characterized in their structure and functioning only as members of a general reference population, one would have to

correlate two variables (namely, a set of stimulus values and a set of response values) paired by being drawn from the same sample of situations or tests, characterized merely as belonging to the class, or 'population,' of living conditions of a particular individual (or category or species). In short, individuals are replaced by situations or tasks (which is to be distinguished from the mere exchange of the role of individuals for tests as in Stephenson's 'inverted' correlation technique). The achievement of a single subject, or even of a single subject in a certain particular attitude, could then be represented by a correlation coefficient based on a variety of test situations involving the stimulus variable in question.

To make the analogy complete, one would, secondly, have to insist on representative sampling of situations or tests, just as in the field of individual differences one has to insist on the representative sampling of individuals from a population to ascertain at least some kind of generality for the result. Proper sampling of subjects is thus replaced by proper sampling of objects or objectives. For general adjustment this would mean a randomization of tasks, a sampling of tests carefully drawn from the universe of the requirements a person happens to face in his commerce with the physical or social environment, as the defining class. For adjustment to, or cognitive attainment of, a single stimulus variable, such as distance, one would have to secure perceptual estimates of distance in a set of situations representative of all the situations and conditions in life which require judgment of, or adjustment to, distance. For each subject, this particular type of perceptual achievement could be represented by the correlation coefficient between measured distances and estimated distances. The more molecular pattern of traditional laboratory experimentation could thus be rounded out to include its molar counterpart, that is an achievement analysis deliberately neglecting the details, even if these details should be relevant in connection with one or the other member of the family of processes mediating this achievement.

An example of the application of the correlation technique to a stimulus response problem can be found in studies on

social perceptual achievement, *e.g.*, when intelligence is to be judged from photographs. Since in typical research of this kind the photographs are not analyzed with respect to their geometric characteristics, the investigation bridges over the mediating layers altogether, in contrast to the purely intra-environmental problems of physiognomics discussed above. It is characteristic of the traditional attitude of psychology that in these studies the sampling problem of social objects has rarely been given due consideration, both regarding sufficient number of social objects as well as the representative character of the sample. And this in spite of the fact that we usually find a sufficient and representative sample of subjects or judges.

On the whole, social perceptual problems have been rooted in the applied disciplines which have not come in too close a contact with the ideology of the 'exact' experimental laboratory psychology and thus have been more openminded to statistics from the beginning. As a methodological demonstration rather than with the purpose of fact finding, the present author has recently undertaken a study in perceptual size constancy in which the correlation technique was applied to a traditional academic stimulus-response problem. Proper sampling was attempted, and an effort was made to throw some light upon the traditional mediation problems of proximal stimulation, besides approaching the achievement problem (3). Purposely, one subject only was used. The person was interrupted frequently during her normal daily activities and asked to estimate the size of the object she just happened to be looking at. Measurements of the object-sizes, which were the distal stimulus variable under consideration, and of their distances from the subject were also taken in each case. These measurements made it possible to compute the relative sizes of the retinal images as well which constituted the most outstanding feature of the mediating proximal stimulus patterns. Estimates of size were found to correlate with object measurements much more closely (between .95 and almost 1.00, depending on method of evaluation, with naive perceptual attitude) than with retinal stimulus size (between .2 and .7). This

result indicates the selective focussing of the organism's response on the distal rather than the proximal variable. In contrast to most experimental studies this result possesses a certain generality with regard to normal life conditions. It furthermore suggests that focussing of psychology upon the proximal and peripheral layer is not the most fruitful thing to do and may lead to an out-of-focus, sterile type of research.

In short, the notion will have to be revised that, while the psychology of individual differences deals with correlations (at least *de facto*), experimental psychology of the stimulus response type deals with, or should strive toward, the uncovering of 'laws' in the strict sense of the word.

As Mises (**9**) has pointed out, law finding and the molecular, microscopic approach are inseparably tied together. In a strict sense, the laws of nature have to be formulated as differential equations, yielding a relationship of the variables in question within an infinitesimally small spatio-temporal region. Their customary macroscopic form is the result of a mathematical integration over time or space, an extension which tacitly implies a number of assumptions about the intervening conditions. Such conditions may be controlled in a sufficiently specific manner in an experiment in which either the possibilities of interference are limited (such as in an optical experiment) and can easily be surveyed, or where the span between the independent and the dependent variable is relatively small in space and time. In this sense the laws of nature are not extremely general, but extremely specific.

III

The tie between the nomothetic and the microscopic attitude is reflected not only in traditional experimental psychology, but also in those more recent endeavors which stress law finding in psychology. The two most outstanding of these attempts are represented by Lewin and Hull.

I agree with Lewin when he makes it clear that there is no place for statistics in a strictly nomothetic, or, as he calls it, systematic discipline (**8**). In fact, not even averages from a large number of cases or repeated observations are in order.

Indeed, those psychologists who have accepted the ideology of accumulated observation have already deviated from the strictly nomothetic path. If all the relevant conditions are known, or rather if all disturbing influences are eliminated, only one observation is needed to ascertain a general law once and forever. Lewin calls this the technique of the 'pure case' and refers to Galileo's study of falling bodies as an example. In an attempt to apply his principles, Lewin has, however, paid the price of an 'encapsulation' of his psychology, at least insofar as theoretical structure is concerned, into what may be called the central layer of the personality. The 'field' within which Lewin is able to predict, in the strict sense of the word, is the person in his life space. But the life space is not to be confused with geographic environment of physical stimuli, nor with actually achieved results in the environment. It is post-perceptual, and pre-behavioral. It represents a cross section in time; yet, in spite of its cross-sectional—or, rather, actualistic—character, it is not considered static (as seems to be, for example, Titchener's old structuralism) but rather dynamic since events are defined as starting points for action. Whether or not, and in which way, action is carried out seems, however, a matter of secondary importance to Lewin. Thus no criterion for directedness of action which would be comparable to, say, Tolman's objective criteria for purposiveness of behavior, are explicitly worked out, and predictions can thus in a strict sense of the word not be tested. Furthermore, Lewin's interest in preparation for action rather than action itself is reflected in his criticism of the use of the concept of achievement, and of the 'historic-geographic' conception of psychology in general, as contrasted with the systematic, nomothetic. All this is only another aspect of what has probably led to his rejection of statistical methods. (It is understood, I hope, that I am referring to the fundamental core and texture of Lewin's theoretical work only and not to his practices which represent a healthy synthesis of his theorizing and the established ways of checking on results.)

Encapsulation into the central layer, with dynamics leading out of it, may be the least harmful of all the limitations which

possibly could be imposed upon psychology: It may actually mean concentration upon the most essential phase in the entire process of life and of its ramifications. It may be the thing psychology has always been really after throughout its history. And there also is a 'dynamic' quality not only in the sense of reaching back to the object (as Brentano and James dreamed of) or forward to the goal (as Lewin undertakes to do) but also in the sense of giving full recognition to cross-sectional interaction within a larger whole, the central system. In his topological psychology, furthermore, Lewin has probably developed the most adequate conceptual tool for dealing with the central layer (which Brentano did not). Yet it is for this methodological perfection that he has paid the price of encapsulation, in that he has furnished but one reference point for all extrinsic dynamics and omitted checks on extrasystemic reference points and thus prevented the actual realization of a truly dynamic outlook.[3]

I should also like to refer to Hull's mathematico-deductive theory of rote learning, as a highly formalized systematic attempt in present day psychology (7). In spirit, the material used goes back to one of the classics of experimental psychology, namely to Ebbinghaus' studies of mechanical memory and thus to a rather elementaristic body of facts. Again the degree of nomothetic perfection which has been reached (though along a quite different line from that of Lewin) appears to be accompanied by a loss of inclusiveness or broadness of content. It is in concepts like that of the habit family hierarchy that Hull has reached the greatest approximation to fundamental structures of life, a fact which is compensated for

[3] In expanding upon his 'principle of contemporaneity' (which, by the way, is characteristic of what the present writer has called 'encapsulation into the central layer') Professor Lewin made the statement that it was obviously of no interest to the psychologist as a psychologist whether or not a rat in a maze would actually get to the food, or whether or not the experimenter was going to give the food, as long as only the rat would start out on its way (or enter the alley). My reply is, that whether or not the animal is actually going to get to food is exactly what we are interested in. Of course, not a single instance would have to count, but general probabilities of arriving at food and the organism's ability to pick up such general probabilities. In the course of the discussion Lewin suggested that the term 'ecology' might be useful in the statistics of the interaction between organism and environment—which, however, Lewin would not want to have included as part of psychology proper.

by the use of a less highly developed systematic apparatus in this latter case.

Somewhat related though much more bound to traditional modes of thought is a large group of psychologists, represented, for example, by Pratt (10), in whose opinion psychology cannot become truly scientific before it has resolved itself into more 'basic' disciplines such as physiology. The basic character of physiology is apparently given by the greater chances of dealing with laws in the strict sense, due to a more molecular character in the approach. Yet, as Woodworth (15) has pointed out, we have to realize that psychology is not a 'fundamental' discipline in this sense of the word.

What seems to be at the bottom of these tendencies is a certain halo effect regarding the concept of exactitude. The principle of methodological physicalism which defines the unity of the sciences should be understood to postulate intersubjective univocality of observation and of communication, not less, but also not more. This univocality is ascertained by the employment of measurement and of mathematical means of communication including such tools as topology. When Watson became the first great exponent of objectivity in psychology, the ideal of exactitude was pressed considerably beyond the purely methodological aspects of physicalism.

Thus, firstly, a point was made about the mechanistic character of psychology as contrasted with vitalistic notions. However, as Carnap has since emphasized, unity of method does not imply unity of laws (4). Molar behaviorists of the present day, such as Tolman, have thus ceased worrying about the problem of explanatory teleology, for which psychology anyway does not seem to be the competent forum, nor capable of furnishing relevant material.[4]

[4] For this reason, correlations observed between organismic variables and certain results of organismic reaction ('ends') as well as certain proximal or distal stimulus variables (objects) should not be branded as entailing undue teleology. In reply to criticism raised by Professor Hull it might be said that statements about such correlations are 'teleological' only in a strictly descriptive and not in the customary explanatory, vitalistic sense. Such terms as 'functionalistic' or 'focal reference' seem to be suitable to characterize such teleologically neutral references to empirically observed relationships between the organism and foci in its environment.

A second, less conceptualized and thus more dangerous bias is based on the confusion between univocality of observation and communication, on the one hand, and the univocality of prediction. It is the latter which leads to the insistence upon law-finding in psychology. Thus, in addition to the mechanistic bias, we have the nomothetic bias. From the standpoint of methodological physicalism, however, a correlation coefficient is just as exact, that is to say, just as public and palpable in its meaning as a law. And it has, it should be kept in mind, considerably more generality, and thus possibilities of prediction, than has an isolated single event such as those studied, in extreme instances, by historians and geographers. And, in a sense, it has even more generality than the 'general' laws of nature which are observed under such meticulously specified conditions.

Another element seems to enter here which may best be characterized by what Lewin (**8**) has called Aristotelian, as contrasted with Galilean, modes of thought (which, however, he himself has apparently failed to avoid in the instance to be referred to here). According to a certain tradition, sciences fall into two categories, nomothetic, or law finding, and idiographic, or referring to individual events. It seems as if psychology would here and there still maintain an Aristotelian, that is to say, an all-or-none attitude toward this dichotomy; if we cannot have the general law, then let us escape into singularity! In a formal sense, however, imperfect correlations fill in the gap between law and isolated fact. Laws allow prediction with certainty, statistics (correlations) predictions with probability,[5] isolated facts allow no prediction at all (unless reference to laws or correlations is tacitly brought in such as in geography where a certain constancy of the crust of the earth is assumed, or in clinical psychology, where a certain consistency of character is anticipated).

The acceptance of ambiguity of prediction as a legitimate and general feature of psychological results will probably meet

[5] In discussing this point, Professor Reichenbach referred to statistics as yielding 'probability laws.' I can see no objection against using the term 'law' in such a sense if only the departure from the nomothetic principle is made clear.

with the same resistance which logicians had to face when they proceeded from a dichotomous true-false alternative to multi-valued logic, or which empirical scientists had to face when developing out of theological and metaphysical stages into the positive stage.[6]

Yet, in establishing the methodological unity of science, it will become increasingly important to emphasize thematic differences. Only when diversity of topic and specific method within unity of general method is fully recognized will we be capable of carrying over the full richness of the psychological problems inherited from introspectionism and other preparatory stages into a thoroughly objectified system. Among the primary obstacles to be removed seem to be the confusions surrounding the concept of exactitude, resulting in its over-expansion. There must be recognition of the fact that there can be no truly molar psychology dealing with the physical relationships of the organism with its environment unless it gives up the nomothetic ideal in favor of a thoroughly statistical conception.[7] In turn, the topical unity of psychology within the constitutional hierarchy of the sciences can be established by specifying, in terms of focal variables, width of span, etc., the kinds of probability relationships maintained between the

[6] Giving up the nomothetic ideal meets with emotional resistance quite comparable to the one encountered when other positions of security and mastery had to be given up in the course of 'Copernican revolutions' such as the dominant position of the earth within the universe, of man within the animal kingdom, or of the Ego within the system of human motivation. Thus one of the two major reasons, listed by Jaensch (until his recent death the most prominent figure of officially sanctioned contemporary German psychology) for the rejection of Gestalt psychology is the fact that Gestalt psychologists particularly emphasize ambiguity in perception, which is considered to be a frightening reflection of their own morbid psychic disposition.

[7] It is in the difference in the use of statistics that we discover why psychology is not just simply "physics of the organism in its environment." The difference exists in spite of the recent emphasis upon the statistical nature of some of the most important sections of physics. It lies in the fact that non-univocality is in physics primarily confined to the 'microscopic' realm and appears to be eliminated, by the sheer weight of large numbers, as long as we remain within phenomena of a macroscopic order. On the other hand, psychology is thoroughly infiltrated with statistics even in the macroscopic sphere, and in fact the more so the more macroscopic, or 'molar,' its problems become. It was indeed for the quantitative expression of one of the most wide-spanning types of relationships, that of inheritance, from generation to generation, of physical and mental characteristics, that correlation statistics were first introduced into the sciences of life. The reason for this introduction of statistics is that relationships on the whole tend to become less foolproof the wider the stretch, in terms of spatio-temporal regions, over which they appear to be maintained.

organism and its environment which are to be included within the scope of psychology.

In the end, this may not even mean a permanent renunciation of at least gradual approximation toward univocality of prediction. Techniques such as multiple correlation or analysis of variance, which consist essentially of a combination of correlations, will increasingly make it possible to narrow down prediction so that we will, at least in a practical sense, come closer and closer to the traditional scientific ideal, the isolation of variables and the establishment of general cause and effect relationships. If we are not to forget the teachings of Hume and John Stuart Mill, we must realize that there is nothing observed but concomitant variation—of greater or lesser relative frequency—and that all analysis of causal textures rests upon this foundation.[8]

[8] In an attempt to show that the relationship of the positions of the three speakers of the meeting was complementary rather than contradictory, the author tried to relate them to well-established disciplines. In visualizing the organism and the organism in its physical (geographical) environment, Lewin's approach seems to be confined to the life center of the organism which may be compared to the study of law and general policy of intra-governmental function in a society. There is indeed lawfulness and consistency within such a system, yet splendid isolation unless contact with other regions is maintained through information, on the one hand, and the executive arm, on the other. Of those external relationships, Hull seems primarily concerned with engineering problems, especially in his attempt to find basic elements such as conditioning to which more complex units could be reduced. Molar behaviorism, in its turn, with its concentration upon vitally relevant if remote historic or geographic variables such as maintenance schedule, time required to reach food, etc., appears in the position of the economist (and hence the term ecological psychology, mentioned above, seems not inappropriate for this kind of approach). An important difference, though one of secondary order, between the type of molar approach represented by Tolman and that proposed by the present writer is that the former seems to put much additional emphasis upon inferences regarding the intra-organismic 'intervening variables' (which brings him close to Lewin), *e.g.*, a hunger drive as inferred from maintenance schedule; whereas the latter would tend, at least in principle, to discard for the moment intervening variables wherever they are not directly accessible. By representing an organism's or species' achievement system in terms of attained objects and results, such a psychology would in a sense be *without* the organism (*i.e.*, would neglect all but a few focal details of organismic structure and intra-organismic processes), yet would let us know much *about* the organism (*i.e.*, its relationships to the environment, in both cognition and action). By exerting actual measurement control mostly about stimuli and results, *i.e.*, about historic or geographic variables, and about central variables only where a direct physiological approach is possible, such a psychology would be the direct counterpart to that represented by Lewin. Namely, it would not be post-perceptual and pre-behavioral, but rather perceptual and behavioral. But in the end, it seems that none of the various aspects just discussed can be dispensed with in a completely rounded-out system of psychology.

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THE PROBLEM OF INTERVENING VARIABLES IN MOLAR BEHAVIOR THEORY[1]

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I. Introduction

There is a striking and significant similarity between the physicalism doctrine of the logical positivists (Vienna Circle) and the approach characteristic of the American behaviorism originating in the work of J. B. Watson (13). Intimately related to both of the above movements are the pragmatism of Peirce, James, and Dewey on the one hand, and the operationism of Bridgman (4), Boring (1), and Stevens (11), on the other. These several methodological movements, together with the pioneering experimental work of Pavlov and the other Russian reflexologists, are, I believe, uniting to produce in America a behavioral discipline which will be a full-blown natural science; this means it may be expected to possess not only the basic empirical component of natural science, but a genuinely scientific theoretical component as well. It is with the latter that the present paper is primarily concerned.

II. Probability Versus Natural Law in the Behavior Sciences

Now, scientific theory is concerned with natural laws. These are conceived as being uniform. Do such isolable uniformities exist in the field of behavior? Of the present panel, Lewin and I believe they do; Brunswik, on the other hand, is convinced that no such uniformities exist, that the best we may hope for is to find correlations among phenomena which will always lack appreciably of being 1.00, possibly ranging mostly

[1] Thanks to the kindness of Brunswik, I have had his revised manuscript before me while preparing the revision of the present paper. Owing to the delay in the revision of my paper, it is doubtful whether either Brunswik or Lewin was able to utilize in any way the copies which were sent them immediately on its completion.

between .40 and .60. Since this disagreement concerns whether something can or cannot be done, it should ultimately be capable of unambiguous decision. If a set of such laws are actually isolated, the question will receive an affirmative answer, but if after a very prolonged effort directed specifically to this task, no such laws are discovered, there will gradually arise an increasingly formidable presumption that the answer to the question is negative.

But since the verdict from trial is very laborious and time-consuming, it is desirable to make as shrewd an estimate of the outcome as possible from evidence now available, because if the quest appears hopeless, all of us may as well give it up, as Brunswik seems already to have done. In this connection Brunswik points to the indisputable fact that under the complex conditions of life, the behavior of organisms is variable. This variability, except possibly in the matter of degree, is not peculiar to the field of behavior. It is easily accounted for without assuming any lack of uniformity in the supposed laws involved: The outcome of a dynamic situation depends upon (1) a set of antecedent conditions and (2) one or more rules or laws according to which, given a certain period of time, these conditions evolve into different conditions or events. It is evident that such a situation implies some degree of uncertainty of dynamic outcome, because we always lack absolutely exact knowledge concerning *conditions*. It does *not* necessarily imply that within the causal segment under consideration stable and uniform sequences may not occur which can be formulated into absolutely uniform rules of molar action. Accordingly, the uncertainty or probabilism of the situation may lie entirely in the conditions and not at all in the rules or laws.

This uncertainty may be illustrated by the following example. In the case of falling bodies, the *law* is represented by the form of the equation,

$$s = \tfrac{1}{2}gt^2,$$

and the *conditions* of its operation are represented by the empirical values of s, g, and t, which must be substituted in the equation if the law is to be verified. In this case the equation is universally believed to be exact, *i.e.*, it is believed that if the

measurement of s on the one hand and of g and t on the other were exact, the equation would hold absolutely when applied to objects falling in a vacuum at the earth's geographical pole; in that case the correlation between the right- and the left-hand sides of the equation would be 1.00. But as a rule, errors of measurement are appreciable, which is to say that it is impossible to know the exact conditions in any given case of a falling body. Therefore, the actual correlation of the values on the respective sides of even the most fully authenticated equations of physics is bound to be something less than 1.00. That this lack of perfect correlation is due to the difficulty of measurement of the conditions rather than to the capriciousness of the alleged law is suggested by the fact that as measurements are improved, repeated, and pooled, the verification of the formula becomes increasingly exact, *i.e.*, the correlation becomes progressively higher. It seems reasonable to suppose that the same situation may exist in the field of behavior.

A second and more serious *a priori* argument against the probable success of the attempt to isolate true natural science laws in the behavior sciences is that any theory of behavior is at present, and must be for some time to come, a molar theory.[2] This is because neuroanatomy and physiology have not yet developed to a point such that they yield principles which may be employed as postulates in a system of behavior theory; consequently, both for empirical and theoretical purposes, behavior must be broken up into relatively coarse causal segments, the interior conditions of which cannot be subjected to observation and measurement. It follows from considerations put forward above that even if the action of the ultimate molecular units of the causal segment were absolutely lawful, the molar outcome of the joint action of the numerous internal elements composing a molar causal segment would vary, because the conditions, being unknown, would presumably vary from one molar situation to another. It would follow that

[2] Molar behavior theory is here opposed to the molecular. The latter would presumably deal with the action of the ultimate nerve cell, the protoplasmic molecules making up the neuron or perhaps the atoms constituting the molecule, or even the electrons, protons, neutrons, etc., constituting the atom. Thus the term *molar* as here used corresponds approximately to the term *macroscopic*, or *coarse-grained*.

molar laws cannot be expected to be absolutely exact. This statement must, I think, be granted.

It would seem, however, that the smaller the molar segment employed, the less will be the uncertainty regarding the conditions and so the smaller will be the role of probabilism in molar dynamic outcomes. By the sagacious pooling of the data obtainable from experimental situations when controlled as well as possible, the net results of the chance variability of the molecular conditions will be largely equalized, and from these there may emerge quantitative laws characteristic of a stable central tendency of the conditions in question.[3] The laws of thermodynamics in gases rest upon exactly this basis; where moderately large samples of gas are involved these laws have a precision which represents a probability of substantially 1.00, something far beyond anything usefully measured by a Pearsonian coefficient of correlation. I conclude, then, that it is not unreasonable to hope for the isolation of both primary and secondary behavioral laws which will hold within a narrow margin of error for averages secured from carefully controlled empirical conditions.[4]

III. Symbolic Constructs Are Widely Used in Behavior Theory

As Brunswik points out, a second indirect effect of the present molar status of the behavior sciences is the resort to

[3] Under the rubric of the oscillation (O) of habit strength below its standard or maximal strength (8, p. 76; 7), there is incorporated in my own theoretical systematization an explicit recognition of a particular phase of this presumptive variability of conditions. This oscillation is tentatively attributed to the random spontaneous firing of the individual nerve cells (7).

[4] I do not at all mean to imply by the above that Brunswik's program of empirical correlational determinations is not without great scientific value. On the contrary, I believe that it is an extremely valuable mode of approach. I do mean, however, that I should expect to derive from molar behavior laws exactly the type of correlations which he finds between the relative potency of the several visual cues in the determination of the verbal responses of his subjects in judging third dimensional distance. Indeed, I am of the opinion that adequate molar laws are even now available which would make this possible; however, such a deduction has not yet been made in rigorous detail, and of course achievements of this kind cannot be claimed with certainty in advance of performance. If Brunswik is prepared publicly to challenge the possibility of such a derivation, I am prepared to attempt it and publish in this journal the outcome, whatever it turns out to be. My program thus does not oppose that of Brunswik, but merely attempts to supplement it.

symbolic constructs or intervening variables. This is a widespread and wholly respectable practice, not only in the behavior sciences but in the physical sciences as well. Actually the physical sciences are molar in much the same sense as are the social sciences, and they are similarly forced to piece out by hypothetical entities the region not directly observable. In addition to molecules, atoms and the various hypothetical subatomic entities such as electrons, protons, etc., are for the most part not only unobserved but presumably unobservable.

In this connection it may be of interest to observe that the use of theoretical constructs or intervening variables is practically universal among writers who have seriously attempted the theoretical systematization of behavior. As the first example, let us take a statement by a leading logical positivist, Rudolph Carnap: "At ten o'clock Mr. A was angry" (**6**, p. 89). Carnap argues that for this statement to have scientific significance, the state of anger must be publicly observable, either directly or indirectly; in this connection he mentions the characteristic behavior which is ordinarily taken as indicating the condition called anger. This behavior, being observable, clearly permits an *indirect* observation of a condition within the body of Mr. A through the conversion, translation, or transformation of the statement about the behavior into a statement about the anger. Thus it becomes apparent that anger, typical of the psychic element in the classical psychology, lies between (1) the antecedent conditions of frustration and what not which precipitated the state, and (2) the observable consequences of the state; *i.e.*, anger is an *unobserved intervening variable*. This dynamic situation is repre-

FIG. 1.

sented conveniently by the diagram of Figure 1, where $\dot{S}$ represents the antecedent conditions which precipitated the anger, $\dot{R}$ represents the observable response, A represents the anger,

f_1 and f_2 represent quantitative functional relationships between $\dot{S}$ and A and between A and $\dot{R}$ respectively. A circle is drawn around A to indicate that it is a symbolic construct, something not directly observable and measurable.

Passing to the more comprehensive statements of Brunswik (2), who is both a logical positivist and a psychologist, we have the situation represented by the diagram of Figure 2, where

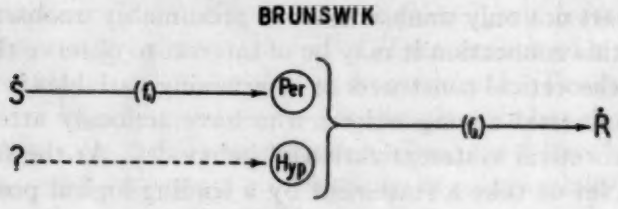

FIG. 2.

$\dot{S}$, $\dot{R}$, f_1, and f_2 mean the same as in Figure 1, Per represents *perception*, and Hyp, *hypothesis*. Here we find not one intervening variable or construct, but two of them operating jointly. Circles are drawn around Per and Hyp to indicate that they are regarded as symbolic constructs.

A third systematic approach is that of Tolman (12). I have attempted to represent this from the present point of view by the diagram shown in Figure 3, where $\dot{S}$, $\dot{R}$, f_1, f_2, and

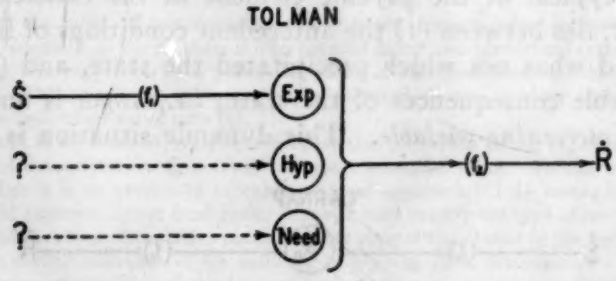

FIG. 3.

Hyp mean the same as above, Exp represents *expectation*, and *Need* is unabbreviated. The circles, as above, indicate the symbolic constructs.

A fourth example of the use of symbolic constructs is found in a study by Buxton (5).[5] It may be represented as shown in Figure 4, where $\dot{S}, f_1, f_2, f_3$, Per, Need, and $\dot{R}$ mean the same as above, Cog represents *cognition*, and Force, *psychological force*.

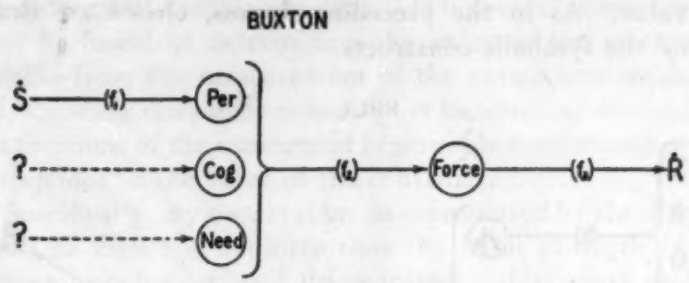

FIG. 4.

It may be noticed that the Buxton diagram has *two* links in its chain of symbolic constructs, rather than the single link represented in the preceding diagrams.

Finally I may mention my own use of symbolic constructs (7). A considerably abbreviated representation of this is shown in Figure 5,[6] where S, R, G, and N represent the stimulus energy, the response, the nature of the reinforcement, and the number of reinforcements of the original reinforcement or learning situation respectively; $\dot{S}$ represents the evocation or stimulus energy; $\dot{s}$, the afferent neural impulse initiated by the receptor discharge; Need, the number of hours of food privation, for example; D, the state of organic drive resulting from the privation; ${}_{S}E_{\dot{R}}$, excitatory or reaction potential;[7] $\dot{R}$, the

[5] At this point I would have preferred to give a representation of Lewin's (10) use of symbolic constructs, but I felt even more uncertain in attempting this than in the case of the representations which were attempted. There seems no doubt, however, that Lewin does employ symbolic constructs both explicitly and on a considerable scale. Perhaps the diagram of Buxton's usage may be considered a rough approximation to that of Lewin, as Buxton in his theorizing seems to have followed Lewin to a considerable degree.

[6] I am indebted to Kenneth W. Spence for the general methodology of representing intervening variables here employed. In this connection see especially page 318 of Spence's Chapter 11, Theoretical interpretations of learning, in *Comparative Psychology* (revised edition), edited by F. A. Moss, New York, Prentice-Hall, Inc., 1942; also see page 11 of Bergmann, G., and Spence, K. W., Operationism and theory in psychology, PSYCHOL. REV., 1941, 46, 1–14.

[7] It is probable that the construct *reaction potential* (${}_{S}E_{\dot{R}}$) corresponds rather closely to Buxton's construct called *Force*, and to Lewin's *vector* concept.

reaction itself; sP_R, the probability of reaction evocation; st_R, the latency of reaction evocation; sA_R, the amplitude of reaction evocation; and snR, the number of unreinforced reaction evocations to extinguish the reaction potential to a subthreshold value. As in the preceding figures, circles are drawn around the symbolic constructs.

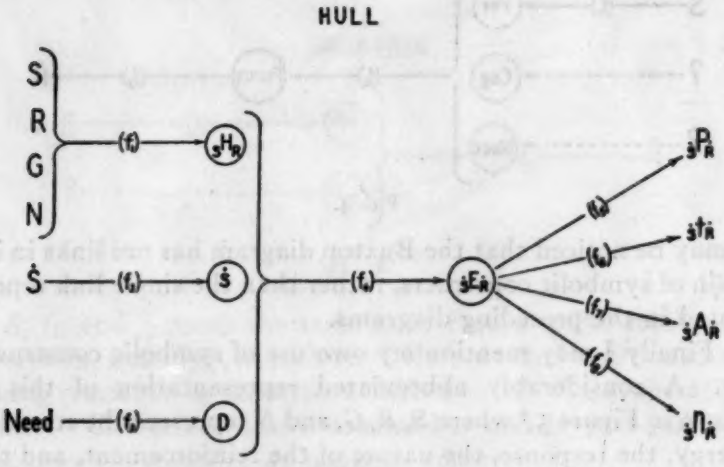

FIG. 5.

It is quite clear from the above examples that the use of symbolic constructs by behavior theorists is both explicit and general.

IV. REQUIREMENTS FOR THE SATISFACTORY USE OF SYMBOLIC CONSTRUCTS

It would appear that Brunswik also has considerable pessimism concerning the utility of symbolic constructs in behavior theory, even though he himself in his visual constancy studies seems at certain moments at least to introduce perception as a variable intervening between the physical stimulation of his subjects and their verbal responses. While I would not go so far as to exclude the use of symbolic constructs, I do believe that their use is attended by serious risks which can be avoided only by taking certain definite precautions.

It is quite clear that in the case of all the above diagrams, symbolic constructs can have nothing more than a rather dubious expository utility unless they are anchored to observable and measurable conditions or events on both the antecedent and consequent sides. It follows that some means must be found of determining the value of the intervening variable from the measurement of the antecedent conditions and, knowing this, some means must be found of determining what amount of the consequent observable conditions or event corresponds to the value of the construct.

Specifically, my own system, as represented by the diagram shown in Figure 5, requires that the habit strength ($_SH_R$), afferent impulse (s), and drive intensity (D) must each be calculable from their antecedent conditions, that the nature and magnitude of the reaction potential ($_SE_R$) must be calculable from the values of $_SH_R$, s, and D taken jointly, and that the nature and magnitude of the several reaction functions ($_SP_R$, $_St_R$, $_SA_R$, and $_Sn_R$) must each be calculable from $_SE_R$. This is necessary because if the values of the constructs are indeterminate, the theory of the relationship between the antecedent and the consequent conditions cannot be verified empirically, and an unverifiable theory has no place in science.

In every case of the proper use of a symbolic construct the calculation must, of course, be performed by means of equations representing the functional relationship in question. In the evolution of my own system considerable effort has been devoted to the derivation of such equations, and numerous tentative ones have been worked out. For example, on the antecedent side (assuming S, R, and G to be practically simultaneous),

$$_SH_R = w - w(10^{-hN}) \qquad (1)$$

where w is an increasing function of the nature and amount of the reinforcing agent (G), and h is an empirical constant of the order .03 (7). Next,

$$_SE_R = f(D) \times f(_SH_R) \qquad (2)$$

where $f(D)$ and $f(_SH_R)$ are rather complex increasing functions of drive intensity and habit strength respectively (7).

As an example of the final link in this chain of functional relationships we have,[8]

$$\$n\dot{R} = .66\$E\dot{R} - 4 \qquad (3)$$

It is evident that this equational mode of anchoring symbolic constructs to objectively observable and measurable antecedent and consequent conditions or phenomena is necessary, because otherwise their values would be indeterminate and the theory of which they constitute an essential part would be impossible of empirical verification. If left permanently in that state the theory would be reduced to something appreciably worse than nonsense. This seems to be what Brunswik charitably calls the "encapsulation" of symbolic constructs.

V. Possibility of Determining Unambiguously the Functional Relationships of Behavioral Unobservables to Other Behavioral Values

It is quite clear that the determination of the functional relationship of an unobservable to anything else cannot be carried out merely by the direct fitting of equations to empirical data, because there can be no empirical data on the side of the relationship represented by the construct. This means that the determination must be somewhat indirect and in part trial-and-error. Such procedures are common in the more mature sciences. Suppose, for example, that a person unacquainted with the law of falling bodies,

$$s = \tfrac{1}{2}gt^2,$$

were given the task of determining it directly by empirical methods. He would first let weights fall repeatedly in a

[8] Tentative equations have now been derived for all of the relationships represented in Figure 5, and the most of the empirical constants involved have been roughly determined. In addition, a number of other functional relationships not represented in the diagram have been worked out in the same tentative manner. It is accordingly possible, given the objective measurable antecedent conditions, S, R, G, H, $\hat{S}$, and D, to calculate the value of the consequent or terminal events, $\$^P\dot{R}$, $\$^t\dot{R}$, $\$^A\dot{R}$, or $\$^n\dot{R}$. These are all given in considerable detail in a book now in press (7). However, an immense amount of research must be performed before the relationships in question can be regarded as satisfactorily determined.

vacuum from various heights, and average the falling time associated with the different distances. These means, with the corresponding distances, would make up two parallel columns of numbers. Then he would begin to try out various simple equational forms, to find one in which the substitution of the t-value would yield the corresponding value in the s-column. He would discover by trial that an exact agreement in any given case could be obtained if the value of t were multiplied by some number called a constant, but this coefficient would need to be different for every pair of values and so could not constitute a true solution to the problem. Similarly, it would be found that if one of these coefficients were kept really constant, s could be derived by multiplying the coefficient by various powers of t. But in that case a different exponent would be required for each pair of empirical s and t values, most of the exponents being fractional. Finally, in the search for simplicity, integral exponents might be tried, among which would be 2. This procedure, of course, would require approximately the *same* coefficient for all pairs of values, the coefficient being what appears in the formula as $g/2$. This finding would constitute a solution.[9]

A criticism of such a procedure might be that it is circular, since the value of the number, $g/2$, follows from the assumption that the t should be squared. The reply to this, of course, is that by the procedure outlined neither the $g/2$ nor the t^2 is assumed at first, but that that particular combination of values, *i.e.*, that particular coefficient and that particular exponent in combination, will yield the empirical value of s, and that *no other possible combination will do so.*

In an exactly analogous manner, if a single symbolic construct stands between two observable and measurable conditions ($\dot{S}$ and $\dot{R}$), and it is found by trial that two particular equations, *and those only*, mediate in conjunction with the

[9] As a matter of fact, once it is postulated that the equation has the general form of

$$s = at^x$$

the values of both a and x can be calculated by means of simultaneous equations from only two pairs of s and t empirical values. Such calculations, of course, will not reveal the exact values of either s or t, but only rather close approximations, because s and t are themselves only approximations.

symbolic construct the precise calculation of $\dot{R}$ from $\dot{S}$, it is submitted that the determination of neither equation may be considered viciously circular. At all events, that is the general method which has been employed in the derivation of the equations given above. The presence of more than one construct (horizontally or vertically, as represented in Figures 1 to 5) considerably complicates the task of working out the equations, but does not change the essential logic.

VI. The Scientific Status of Symbolic Constructs When Unambiguously Determined

At this point in the discussion it is easy to imagine a rock-ribbed positivist like my friend Woodrow (14) saying, "If you have a secure equational linkage extending from the antecedent observable conditions through to the consequent observable conditions, why, even though to do so might not be positively pernicious, use several equations where one would do?" It is quite true that in my own equations (1) can be substituted in (2), and (2), thus amplified, can be substituted in (3); in which case we would have a single equation relating the antecedent conditions directly to the consequent ones, though it would be exceedingly complex. Indeed, if the situation were as simple as this question implies, there would hardly be any point in the derivation of multiple equations where one would fit the entire situation equally well. It does not seem to me that any single equation would really fit the situation equally well.

In reaching a decision concerning this question, two considerations should be kept in mind. One is that if the antecedent and consequent values have been determined with great precision, only one particular equation will *exactly* satisfy them and that equation would, it is assumed, be no other than the one resulting from the telescoping of the three equations as above suggested. In other words, for the multiple equations to be justified, the single equation giving the best attainable fit to the empirical data would be found in fact to contain within itself in some form or other the mathematical equivalents of the various equations linking the observable and the hypothetical unobservable elements of the situation.

A second and more decisive consideration involved in reaching a decision in this matter is that the occasion of the action of $\dot{S}$, $\dot{R}$, G, and N may be very remote temporally from that of the action of S and the Need. While it is perfectly possible to put into a single equation the values of events which occur at very different times, it is hard to believe that an event such as a stimulation in a remote learning situation can be causally active long after it has ceased to act on the receptors. I fully agree with Lewin that all the factors alleged to be causally influential in the determination of any other event must be in existence at the time of such causal action. I believe that it is some such consideration as this which has led to the universal common-sense use of the concept of habit; in my system $_{S}H_{\dot{R}}$ is merely a quantitative representation of the perseverative after-effects of the no-longer-existent compound events represented by $\dot{S}$, $\dot{R}$, G, and N. It follows that the value of $_{S}H_{\dot{R}}$ will need to be calculated separately before the influence of $\dot{S}$, $\dot{R}$, G, and N can be combined with that of the reaction-evocation situation represented by S and the Need. It is an obvious convenience to be able to concentrate the influence of four quantitative variables, such as $\dot{S}$, $\dot{R}$, G, and N, into a single numerical value or index. Since this index is not $\dot{S}$ or $\dot{R}$ or G or N, but something distinct from each, convenient symbolic manipulation demands a distinct symbolic representation, *i.e.*, $_{S}H_{\dot{R}}$. Semantic exigencies also require the use of a name or term; the universally used word *habit* has been adapted to this use by quantitative redefinition.

In an analogous manner it is hoped that the factors which determine primary drive (such as the number of hours of food or water privation, the intensity of an electric shock, or the number of degrees of temperature below or above the optimum) may each also be convertible into a standard index of motivation (D), presumably by means of a different equation for each type of need. If wholly successful, such a treatment of primary motivation would yield a standard D or drive value in each case which could be *substituted without distinction in the same single equation* (2) *yielding* $_{S}E_{R}$ in such a way as to mediate within the limits of errors of measurement the observable

behavioral phenomena, ${}_{S}P_{R}$, ${}_{St}R$, etc. If a program like that just outlined should turn out to be successful, it would practically require the use of multiple equations and so of the special signs which we have called symbolic constructs.

It is possible, of course, that animals are so constituted that a program such as has been implicitly outlined in the present paper can never be successfully carried through. Whatever may be our several *a priori* prejudices in the matter of the probable outcome of such a program, I think we should be able to agree upon two propositions relating to it. One is that if such equations could be found, they would greatly facilitate the theoretical integration of the behavior sciences as well as their practical application to the needs of man. The second proposition is that the only wholly convincing way of demonstrating this possibility is for some person or, preferably, some group of persons, actually to derive the equations in question and show that they do indeed mediate rather precisely the derivation of consequent behavior from the measured complex antecedent conditions as outlined above. Naturally such work will be done by those who hope for its achievement and believe in its possibility; those who do not share these hopes and beliefs will engage in other ventures. In the end, if the equations are derived successfully they will be available for all to use, and both groups should rejoice in the contemplation of the advantages ultimately to accrue. On the other hand, if the quest proves unsuccessful, those engaged in it will have had the thrill of high adventure and the satisfaction of having made a worthy effort.

Supplementary Note

Professor Lewin's manuscript did not become available to me until after the preceding portion of the present contribution to the symposium had been sent in. I have accordingly availed myself of the editor's permission to comment briefly in this note.

I am in complete agreement with Lewin when he remarks, "In other words, field theory can hardly be called a theory in the usual sense" (though possibly my understanding is not exactly what he intends). I believe the statement quoted above to be literally true when taken in conjunction with the methodology favored by Lewin.

I do not believe it to be true of field theory in general, nor in particular of the view of it put forward in a passage from Hilgard and Marquis, a portion of which is quoted by Lewin.[10]

The main reason why I believe the methodology favored by Lewin cannot yield a satisfactory natural-science theory of behavior, field or otherwise, is that his field when secured is primarily subjective and his preferred method of securing it is primarily introspective. As I read him, experimental extinction occurs because 'the individual catches on,' conditioned reactions are evoked in part because of a concurrent 'expectation,' and so on. Moreover, when attributed to inarticulate organisms, subjective entities such as 'expectancy,' 'catching on,' 'life space,' 'subjective probability,' and the like would seem to degenerate to sheer anthropomorphism; the investigator in effect projects himself into the rat, the cat, the dog, the monkey, the ape, or the young child, and says, "If I were in that situation I would perceive, cognize, feel, think, or hypothesize so and so."

The main trouble with the subjective and anthropomorphic methodology which Lewin seems to say he favors is that the entities which it yields are not really measurable, whereas the satisfactory verifiability of natural-science theory requires rather precise quantification; in so far as a theory or hypothesis is incapable of verification, it falls short of the scientific ideal. For example, the trouble with such a concept as expectancy, as employed by Hilgard and Lewin, is that when we attempt to verify the hypothesis in which it appears we cannot tell how much expectancy to expect; neither do we know the magnitude of the reaction which the expectancy is expected to mediate. This lack of determinate quantitative relationship to objectively observable and measurable states and events is, I

[10] The entire passage is as follows:

"As I see it, the moment one expresses in any very general manner the various potentialities of behavior as dependent upon the simultaneous status of one or more variables, he has the substance of what is currently called field theory. My habit-family hypothesis is presumably a field principle in this sense. It is probable that my equation expressing the goal gradient hypothesis (gradient of reinforcement) and even the one expressing the generalization gradient (gradient of irradiation of Pavlov) might also qualify as bits of field theory. On the other hand, some might object to this usage on the ground that equations of this nature are not peculiar to gravitational or electromagnetic fields but are almost universal in natural science theory. Moreover the variables and the constants, together with their inter-relationships, which have so far appeared in equations expressing behavior theories, seem to differ radically from those which appear in the equations of physical field theories. The notable lack of such equations in current Gestalt field formulations, while paradoxical, is probably symptomatic. For such reasons I prefer to be rather sparing in my use of the expression 'field theory'; if one has the substance, the use of the term is scientifically unimportant however valuable it may be for purposes of propaganda. But if one means by field theory what I have indicated, I am all for it and I see no inherent disagreement on this point between stimulus-response theory and Gestalt theory."

believe, what Brunswik means by 'encapsulation.' If so, I quite agree with his objections to it.

It seems inevitable that as the behavior sciences evolve, the relationships where multiple causes are involved will be expressed more and more precisely in the form of equations; if someone wishes to liken this tendency to the phase in the evolution of gravitational and electromagnetic science, I have no objection. The main thing is that the task be accomplished, that the joint causal relationships be determined with a high degree of precision. It is hardly to be doubted that the behavior sciences are rapidly moving in this direction. On the other hand, in every culture there are to be found individuals who prefer their portraits to be artistically out of focus. Similarly, there will always be found scientists who prefer their theories to be vague, who fear that exact formulation will freeze them or constitute a kind of harness restricting the freedom of the workers. But two swallows do not make a spring. I cannot agree that American psychology is moving in the direction of more introspection, subjectivity, anthropomorphism, or vagueness.

It may be, of course, that I have totally misunderstood Lewin's meaning. I would like to believe that he has not been advocating the things to which I have taken exception above, because, if generally accepted, they would have an unfortunate effect on the development of the behavior sciences.

Remarks to Mr. Hull's Supplementary Note by Kurt Lewin

Professor Hull states, "It may be, of course, that I have totally misunderstood Lewin's meaning. I would like to believe that he has not been advocating the things to which I have taken exception above." Happily, I feel sure he did misunderstand me. I have never expressed the opinion "that American psychology is moving in the direction of more introspection, subjectivity, anthropomorphism, or vagueness," and I would not agree to such a statement about psychology—American or otherwise.

Mr. Hull may not have realized that my little sketch, which describes a typical reconditioning set-up in common language, was not meant to be more than a 'description.' As Mr. Hull knows, if I present 'theories' they are usually expressed in mathematical terms of geometrical regions, vectors, and equations. In other words, in regard to theory we both feel compelled and justified to use a straight scientific terminology. But I have been so frequently accused of using too highbrow or too physicalistic a language that outside the level of 'explanatory theory' I try to keep to the common language—even if speaking of conditioning situations. I, too, have been slightly bothered by certain terminologies; for instance, why use the term 'goal response' instead of the perfectly good common term

'consumption' if one actually does not mean all types of reactions to a goal, but just consummatory actions? There is a danger that the use of a terminology might put up the appearance of a theory when there is but an uncommon descriptive term inserted for a common term. It might be quite healthy for widening the range of the theories which should be considered as possible explanations for certain phenomena if one realized that these phenomena can be described in various ways.

Hull says: "For example, the trouble with such a concept as expectancy, as employed by Hilgard and Lewin, is that when we attempt to verify the hypothesis in which it appears we cannot tell how much expectancy to expect; neither do we know the magnitude of the reaction which the expectancy is expected to mediate"; but looking over the literature on the Level of Aspiration, Hull would quickly see that reliable quantitative data about 'expectancy' can be obtained, that the conditions for changing expectancy and the 'magnitude of reaction' can be studied, at least in human beings. I hope he will be pleased to see that it is even possible to make rather specific theoretical predictions about expectancy and to prove them experimentally (5).[11] Certainly it is a legitimate question to ask at what age level or what phylogenetic level one can adequately speak of 'expectation,' how behavior differs in case of certain expectation or non-expectation, etc. There may be differences of opinion among animal psychologists as to whether or not today a satisfactory operational definition can be given for this term in regard to animals. These, however, are technical problems and no question of principle is involved.

I suppose Mr. Hull is not likely to be persuaded that the term 'expectation' is at least as well defined operationally—and, in my mind, better defined conceptually—as many terms in psychology of perception or conditioning which he accepts. Neither am I likely to be persuaded that some of the alleged 'stimuli' referred to in conditioning are not altogether too vague, unrealistic, and inadequate, in spite of their emphasized front of 'objectivity.' My main criticism against some of these concepts or theories has been their lack of analytical clarity (12), and I was pleased to notice recent changes in this direction. These criticisms of specific points, however, did not make me feel justified to accuse anyone of 'advocating' vague, subjective and unanalytical thinking. Mr. Hull knows as well as I do that in actual research we all have to give operational definitions, that no one yet has been able to eliminate from the operational definitions in psychology what so wittingly is called 'language reaction,' that we all have to be ready to prove the relia-

[11] References in Dr. Lewin's 'Remarks' are to be found in the bibliography of his original article, pp. 309–310.

bility of our observational instruments, and that all our data must be ready to face the same scrutinizing criteria for their significance. Why, then, bring up the problem of "advocating vagueness, introspection, subjectivity, anthropomorphism"? Frankly, I have been bored for a decade by the dispute about these fetishes of a theological controversy which in actual research has died long ago. I hope that this point of satiation will be reached by all of us soon because that would help considerably in straightening out the real issues.

These issues behind Mr. Hull's note seem to be two:

1. Are we ready to apply in an unemotional, unbiased and fair manner the same test of 'scientific objectivity'—on which we all pretty well agree—to such terms as 'expectation,' 'hope,' and even to such cultural or sociological terms as 'generosity,' 'fairness,' or 'friendship' that we apply to terms like 'nonsense syllables,' 'goal,' or 'goal response'?

2. Some aspects of the classical theory of association and conditioned reflex have correctly been characterized as 'psychology without organism.' I suppose that we all agree in principle that psychology cannot leave out the organism. But some of us seem to be too timid to believe that a real science of the organism is possible. They prefer the most peculiar round-about routes to a recognition of the simple principle that all psychological concepts, including 'physical stimuli,' have finally to be expressed in terms which relate to the organism in its particular state. These psychologists seem to suffer from a too generalized concept of subjectivity and to be haunted by the fear that someone will call them subjective in the sense of unscientific if they think in terms of the specific situation of the particular organism. I had felt that in Mr. Hull's and my original articles we had overcome some unnecessary misunderstandings in this matter.

Finally, a word about Hull's remark on the poor way in which such field theorists, as I, are handling field theory. I suppose that Mr. Hull will understand that even the cry "Let's take field theory away from the field theorists" would be sweet music for the latter. I am even optimistic enough to believe that, in time, Hull and I will agree as to the main characteristics of field theory, in particular that this is a method of analyzing causal relations and constructing theories and concepts rather than a 'theory' in the usual sense of the term. In the meantime what counts is that after a decade of refutation, psychology is turning more and more toward field theory.

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DEFINING THE 'FIELD AT A GIVEN TIME'[1]

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I. Field Theory and the Phase Space

The history of acceptance of new theories frequently shows the following steps: At first the new idea is treated as pure nonsense, not worth looking at. Then comes a time when a multitude of contradictory objections are raised, such as: the new theory is too fancy, or merely a new terminology; it is not fruitful, or simply wrong. Finally a state is reached when everyone seems to claim that he had always followed this theory. This usually marks the last state before general acceptance.

The increasing trend toward field theory in psychology is apparent in recent variations of psychoanalysis (Kardiner, Horney) and also within the theory of the conditioned reflex. This trend makes the clarification of the meaning of field theory only the more important, because, I am afraid, those psychologists who, like myself, have been in favor of field theory for many years have not been very successful in making the essence of this theory clear. The only excuse I know of is that this matter is not very simple. Physics and philosophy do not seem to have done much analytical work about the meaning of field theory that could be helpful to the psychologist. In addition, methods like field theory can really be understood and mastered only in the same way as methods in a handcraft, namely, by learning them through practice.

Hilgard and Marquis (7), in a recent publication, quote from a letter of Clark Hull the following sentence: "As I see it, the moment one expresses in any very general manner the

[1] This is the third paper given at a Symposium on Psychology and Scientific Method held as part of the Sixth International Congress for the Unity of Science, University of Chicago, September, 1941. The first paper is by Egon Brunswik and the second by C. L. Hull.

various potentialities of behavior as dependent upon the simultaneous status of one or more variables, he has the substance of what is currently called field theory."

It is correct that field theory emphasizes the importance of the fact that any event is a resultant of a multitude of factors. The recognition of the necessity of a fair representation of this multitude of interdependent factors is a step in the direction toward field theory. However, this does not suffice. Field theory is something more specific.

To use an illustration: Success in a certain sport may depend upon a combination of muscular strength, velocity of movement, ability to make quick decisions, and precise perception of direction and distance. A change in any one of these five variables might alter the result to a certain degree. One can represent these variables as five dimensions of a diagram. The resultant of any possible constellation of these factors for the amount of success can be marked as a point in the diagram. The totality of these points then is a diagrammatic representation of this dependence, in other words, of an empirical law.

Physics frequently makes use of such representation of a multitude of factors influencing an event. To each of certain properties, such as temperature, pressure, time, spacial position, one dimension is coordinated. Such a representation in physics is called 'phase space.' Such a phase space may have twenty dimensions if twenty factors have to be considered. A phase space is something definitely different from that three-dimensional 'physical space' within which physical objects are moving. In the same way the psychological space, the life space or psychological field, in which psychological locomotion or structural changes take place, is something different from those diagrams where dimensions mean merely gradations of properties.

In discussing these questions with a leading theoretical physicist, we agreed that the recognition of a multitude of factors as determining an event, and even their representation as a phase space, does not presuppose field theory. In psychology, Thurstone's factor analysis deals with such relations

of various factors. Any character profile recognizes the multitude of factors. Field theorists and non-field theorists can both avail themselves of these useful devices, but not everybody who uses them is therefore a field theorist.

What is field theory? Is it a kind of very general theory? If one proceeds in physics from a special law or theory (such as the law of the free-falling body) to more general theories (such as the Newtonian laws) or still more general theories (such as the equations of Maxwell), one does *not* finally come to field theory. In other words, field theory can hardly be called a theory in the usual sense.

This fact becomes still more apparent when we consider the relation between the correctness or incorrectness of a theory and its character as a field theory. A special theory in physics or psychology may be a field theory, but nevertheless wrong. On the other hand, a description of what Hans Feigl calls an "empirical theory on the lowest level" may be correct without being field theory (although I do not believe that a theory on the higher levels of constructs can be correct in psychology without being field theory).

Field theory, therefore, can hardly be called correct or incorrect in the same way as a theory in the usual sense of the term. *Field theory is probably best characterized as a method:* namely, a method *of analyzing causal relations and of building scientific constructs.* This method of analyzing causal relations can be expressed in the form of certain general statements about the 'nature' of the conditions of change. To what degree such a statement has an 'analytical' (logical, *a priori*) and to what degree it has an 'empirical' character do not need to be discussed here.

II. The Principle of Contemporaneity and the Effect of Past and Future

One of the basic statements of psychological field theory can be formulated as follows: Any behavior or any other change in a psychological field depends only upon the psychological field *at that time.*

This principle has been stressed by the field theorists from the beginning. It has been frequently misunderstood and interpreted to mean that field theorists are not interested in historical problems or in the effect of previous experiences. Nothing can be more mistaken. In fact, field theorists are most interested in developmental and historical problems and have certainly done their share to enlarge the temporal scope of the psychological experiment from that of the classical reaction-time experiment, which lasts only a few seconds, to experimental situations, which contain a systematically created history through hours or weeks.

If a clarification of the field theoretical principle of contemporaneity could be achieved, it would, I feel, be most helpful for an understanding among the various schools in psychology.

The meaning of this far-reaching principle can be expressed rather easily by referring to its application in classical physics.

A change at the point x in the physical world is customarily characterized as $\frac{dx}{dt}$; that is to say, as a differential change in the position of x during a differential time-period dt. Field theory states that the change $\frac{dx}{dt}$ at the time t depends only on the situation S^t at that time t (Figure 1).

$$\frac{dx}{dt} = F(S^t) \tag{1}$$

It does not depend, in addition, on past or future situations. In other words, the formula (1) is correct, but not the formula (1a).

$$dx = F(S^t) + F^1(S^{t-1}) + \cdots + F^2(S^{t+1}) + \cdots \tag{1a}$$

Of course, there are cases in physics where one can state the relation between a change and a past situation S^{t-n} (where $t - n$ is a time not immediately preceding t; $|t - n| > dt$). In other words, there are occasions where it is technically possible to write:

$$\frac{dx}{dt} = F(S^{t-n}) \tag{2}$$

However, that is possible only if it is known how the later situation S^t depends on the previous situation S^{t-n}; in other words, if the function F in the equation

$$S^t = F(S^{t-n}) \tag{3}$$

is known. Such knowledge presupposes usually (a) that both situations are 'closed systems' which are genidentic (11); (b) that the laws are known which deal with the change of all points of the previous situation S^{t-n} and also the laws dealing with the changes in the situations between the previous situation S^{t-n} and the later situation S.

The meaning of linking a change to a past situation by formula (2) might be clarified best by pointing out that it is possible in a similar way to link a present change to a future situation S^{t+n} and to write:

$$\frac{dx}{dt} = F(S^{t+n}) \tag{2a}$$

This is possible whenever we have to deal with a 'closed system' during the time-period t until $t + n$, and if the laws of the on-going changes during this period are known.

The possibility of writing this functional equation does not mean that the future situation S^{t+1} is conceived of as a 'condition' of the present change $\frac{dx}{dt}$. In fact, the same $\frac{dx}{dt}$ would occur if the closed system would be destroyed before the time $(t + n)$. In other words, the change $\frac{dx}{dt}$ depends on the situation (S^t) at that time only (in line with formula [1]). The technical possibility of expressing this change mathematically as a function of a future or a past time does not change this fact.[2]

The equivalent to $\frac{dx}{dt}$ in physics is the concept 'behavior' in psychology, if we understand the term behavior to cover

[2] Frequently an occurrence is said to be caused by the 'preceding conditions.' This term seems to have been misunderstood by psychologists to refer to a distant past situation (S^{t-n}), although it should refer to the present situation, or at least to the 'immediately preceding situation' (S^{t-dt}). We will come back to this question.

any change in the psychological field. The field theoretical principle of contemporaneity in psychology then means that the behavior b at the time t is a function of the situation S at the time t only (S is meant to include both the person and his psychological environment),

$$b^t = F(S^t) \tag{4}$$

and not, in addition, a function of past or future situations S^{t-n} or S^{t+n} (Fig. 2). Again, it is possible to relate the behavior b indirectly to either a past situation (S^{t-n}) or a future situation (S^{t+n}); but again, this can be done only if these situations are closed systems, and if the changes in the intermediate periods can be accounted for by known laws. It seems that psychologists are increasingly aware of the importance of this formula.

III. How to Determine the Properties of a Field at a Given Time

If one has to derive behavior from the situation at that time, a way has to be found to *determine* the character of the 'situation at a given time.' This determination implies a number of questions which are, I think, interesting both psychologically and philosophically.

To determine the properties of a present situation or—to use a medical terminology—to make a diagnosis, one can follow two different procedures: One may base one's statement on conclusions from history (*anamneses*), or one may use diagnostic *tests of the present.*

To use a simple example: I wish to know whether the floor of the attic is sufficiently strong to carry a certain weight. I might try to gain this knowledge by finding out what material was used when the house was built ten years ago. As I get reliable reports that good material has been used, and that the architect was a dependable man, I might conclude that the load probably would be safe. If I can find the original blueprints, I might be able to do some exact figuring and feel still more safe.

Of course, there is always a chance that the workmen have actually not followed the blueprints, or that insects have weakened the woodwork, or that some rebuilding has been done during the last ten years. Therefore, I might decide to avoid these uncertain conclusions from past data and to determine the present strength of the floor by testing its strength now. Such a diagnostic test will not yield data which are absolutely certain; how reliable they are depends upon the quality of the available test and the carefulness of testing. However, the value of a present test is, from the point of view of methodology, superior to that of an *anamneses*. An *anamneses* includes logically two steps: namely, the testing of certain properties in the past (of the quality, size, and structure of the woodwork) and the proof that nothing unknown has interfered in the meantime; in other words that we have to deal with a 'closed system.' Even if a system is left untouched by the outside, inner changes occur. Therefore, in addition, the laws governing these inner changes have to be known (see above) if the properties of a situation are to be determined through an *anamneses*.

Medicine, engineering, physics, biology are accustomed to use both methods, an inquiry into the past and a test of the present. But they prefer the latter whenever possible.[3]

Psychology has used diagnosis by *anamneses* rather excessively, particularly in classical psychoanalysis and other clinical approaches to problems of personality. Psychology of perception and psychology of memory have been relatively free from the historical type of diagnosis. Experimental psychology, on the whole, has shown a progressive trend toward testing the present situation.

The method of determining the properties of a situation (S^t) by testing them at that time t avoids the uncertainties of

[3] There are cases where a historical procedure is preferable. For instance, the hunger of a rat can probably be better determined by the duration of starvation than by a physiological or psychological test of the hunger at the time t. This conclusion from the past to the present can be made, however, only during periods and in settings where a 'closed system' (no interference from outside) can be enforced; *e.g.*, for animals which during this period do the same amount of work, which have been on a known diet, etc. The difficulties of this type of control have lead Skinner (19) to link the problem of drive strength to properties of present consumption.

historical conclusions. It does not follow, however, that this method eliminates considerations of time-periods altogether. A 'situation at a given time' actually does not refer to a moment without time extension, but to a certain time-period. This fact is of great theoretical and methodological importance for psychology.

It may be helpful to go back for a moment to the procedure in physics. If the vertical lines in Fig. 1 represent the so-

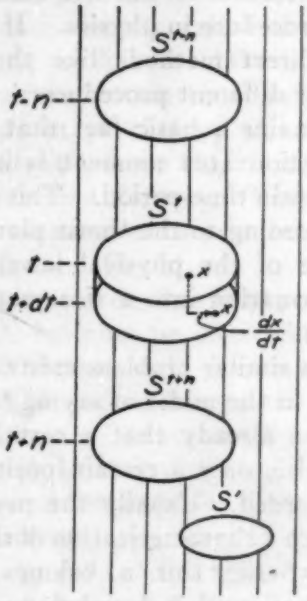

FIG. 1. S during $t - n$ until $t + n$ is a 'closed system'; but S is not genidentic with S'. $\frac{dx}{dt}$ indicates the velocity of x.

called physical 'world-lines,' a 'situation' means a cut through these lines at a given time t. A description of such a situation has to include (1) the relative position of the parts of the field at that time; (2) the direction and the velocity of the changes going on at that time. The first task is fulfilled by ascribing certain scalar values to the different entities; the second, by ascribing certain vectors to them. The second task contains a difficulty which I would like to discuss.

To describe the direction and velocity of a change going on at a given moment, it is necessary to refer to a certain period of events. Ideally, a time-differential should suffice for such determination. Actually, one has to observe a macroscopic time-interval or at least the position at the beginning and at the end of such interval to determine that time-differential. In the simplest case the velocity at a given time is assumed to equal the average velocity during that macroscopic time-interval. I will not attempt to follow up the details of this procedure in physics. If sufficient laws are known, certain indirect methods like those based on the Dopler effect permit different procedures.

However, it remains a basic fact that the adequate description of a situation at a moment is impossible without observation of a certain time-period. This observation has to be interpreted (according to the 'most plausible' assumption and our knowledge of the physical laws) in a way which permits its transformation into a statement of the 'state of affairs at the time *t*.'

In psychology a similar problem exists. The person at a given time may be in the midst of saying 'a.' Actually such a statement implies already that a certain time-interval is observed. Otherwise, only a certain position of mouth and body could be recorded. Usually the psychologist will not be satisfied with such a characterization of the ongoing process. He likes to know whether this 'a' belongs to the word 'can' or 'apple' or to what word it does belong. If the word was 'can,' the psychologist wants to know whether the person was going to say: "I cannot come back" or "I can stand on my head if I have to." The psychologist even likes to know whether the sentence is spoken to an intimate friend as a part of a conversation about personal plans for the future or whether this sentence is part of a political address and has the meaning of an attempt to retreat from an untenable political position.

In other words, an adequate psychological description of the character and the direction of an ongoing process can and has to be done on various microscopic and macroscopic levels.

To each 'size of a unit of behavior' a different 'size of situation' can be coordinated. That the individual in our example is saying "a," can be made sure without taking into account much of the surrounding of the individual. To characterize the sentence as a part of a political retreat, much more of the surrounding has to be considered.

Without altering the principle of contemporaneity as one of the basic propositions of field theory, we have to realize that to determine the psychological direction and velocity of behavior (*i.e.*, what is usually called the 'meaning' of the psychological event), we have to take into account in psychology as in physics a certain time-period. The length of this period depends in psychology upon the scope of the situation. As a rule, the more macroscopic the situation is which has to be described the longer is the period which has to be observed to determine the direction and velocity of behavior at a given time (Fig. 2).

In other words, we are dealing in psychology with 'situational units' which have to be conceived of as having an extension in regard to their field dimensions and their time dimensions. If I am not mistaken, the problem of time-space-quanta, which is so important for modern quantum theory in physics (17), is methodologically parallel (although, of course, on a more advanced level) to the problem of 'time-field-units' in psychology.

The concept of situations of different scope has proved to be very helpful in solving a number of otherwise rather puzzling problems. Tolman (20), Muenzinger (16), and Floyd Allport (1), have stressed that a psychological description has to include the macroscopic as well as the microscopic events. Barker, Dembo, and Lewin (2) distinguish and treat mathematically three sizes of units of processes and corresponding sizes of situations. They have handled certain problems of measuring the strength of frustration during extended periods by referring to overlapping situations in regard to two different sizes of time-field-units. Lippitt and White (15), in their study of social atmosphere, distinguish still larger periods of events. They have shown that the beginning

and end of these macroscopic units can be determined rather precisely and with very satisfactory reliability. However, I will not discuss these questions here where we are interested in methodological problems only.

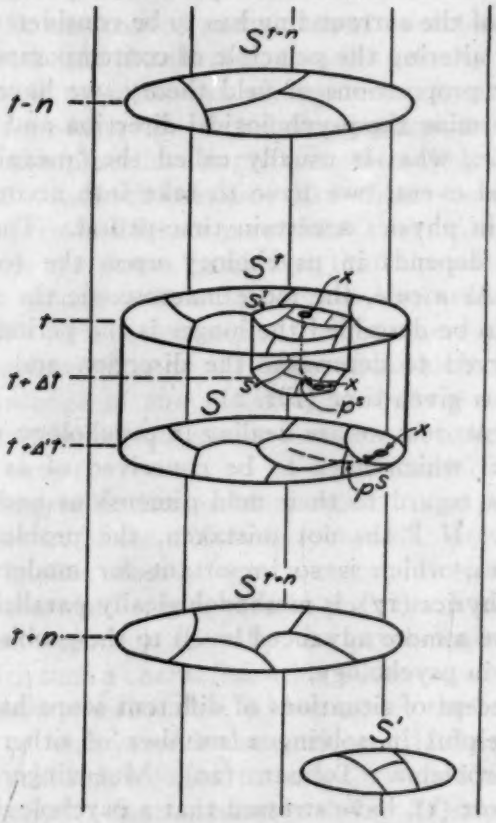

FIG. 2. S during $t - n$ until $t + n$ is a 'closed system'; but S is not genidentic with S'. $S^{t,t+\Delta t}$ is a small time-field-unit which extends over a relatively small area and includes the relatively small time-period t until $t + \Delta t$. $S^{t,t+\Delta' t}$ is a larger time-field-unit covering a larger area and including the longer period t until $t + \Delta' t$. p^s and p^S indicate the change in position of x during the small and the large time unit.

IV. The Psychological Past, Present, and Future as Parts of a Psychological Field at a Given Time

The clarification of the problem of past and future has been much delayed by the fact that the psychological field

which exists at a given time contains also the views of that individual about his future and past. The individual sees not only his present situation; he has certain expectations, wishes, fears, daydreams for his future. His views about his own past and that of the rest of the physical and social world are often incorrect, but nevertheless constitute, in his life space, the 'reality-level' of the past. In addition, a wish-level in regard to the past can frequently be observed. The discrepancy between the structure of this wish- or irreality-level of the psychological past and the reality-level plays an important role for the phenomenon of guilt. The structure of the psychological future is closely related, for instance, to hope and planning (**2**).

Following a terminology of L. K. Frank (**6**), we speak of 'time perspective' which includes the psychological past and psychological future on the reality-level and on the various irreality-levels. The time perspective existing at a given time has been shown to be very important for many problems such as the level of aspiration, the mood, the constructiveness, and the initiative of the individual. Farber (**4**) has shown, for instance, that the amount of suffering of a prisoner depends more on his expectation in regard to his release, which may be five years ahead, than on the pleasantness or unpleasantness of his present occupation.

It is important to realize that the psychological past and the psychological future are simultaneous parts of the psychological field existing at a given time *t*. The time perspective is continually changing. According to field theory, any type of behavior depends upon the total field, including the time perspective at that time, but not, in addition, upon any past or future field and its time perspectives.

It may be illustrative to consider briefly from this field theoretical point of view the methodological problems connected with one of the basic concepts of the conditioned reflex theory, namely, the concept of 'extinction.' An individual has experienced that after a certain stimulus, let us say the ringing of a bell, food will appear. Being hungry, the individual eats. After a number of such experiences, the indi-

vidual will show certain preparatory actions for eating as soon as the eating bell rings. The individual is then said to be 'conditioned.' Now, the situation is secretly changed by the experimenter and the eating bell is not followed by food. After a while the individual catches on and does not show the preparatory action for food when the bell rings. This process is called 'extinction.'

'Habits' of a person at a given time can and have to be treated as parts of the present field. Whether they should be represented partly as cognitive structure or resistance to change of cognitive structure, partly as a building up or fixation of valences (13), or whether they have to be conceptualized in other ways is not a problem here. Habits of action (18, 14), as well as of thinking, are dealt with in field theoretical research. They are closely related to problems of ideology (9) and expectation.

As Tolman (20), Hilgard and Marquis (7), and others have correctly pointed out, conditioning as well as extinction are both related to changes in the reality level of the psychological future. Field theorists have to distinguish in regard to conditioning and extinction two types of problems. The one type deals with such a question as how expectation is affected by perception on the one hand, and memory on the other. What changes in the perceived structure of the psychological present lead to a change in the structure of the psychological future, and what are the laws governing the interdependence of these two parts of the psychological field? The studies on level of aspiration have provided some knowledge about the factors which influence the structure of the future reality-level. Korsch-Escalona (10) has made a step toward a mathematical treatment of the effect of the future reality-level on the forces which govern present behavior. Study of the level of aspiration has also given us considerable insight into the effect of the psychological past (namely of previous success or failure) on the psychological future. This question is obviously closely related to extinction.

The methodological position of these types of problems is clear: They deal with the interdependence of various parts of

the psychological field existing at a given time t. In other words, they are legitimate field theoretical questions of the type $b^t = F(S^t)$.

The second type of questions, treated in the theory of conditioned reflex, tries to relate a later situation S^4, (for instance, during extinction) to a previous situation S^1 during learning or to a number of similar or different previous situations S^1, S^2, S^3, $\cdots$: it relates behavior to the number of repetitions. In other words, these questions have the form $b^t = F(S^{t-n})$ or $b^t = F(S^{t-n}, S^{t-m}, \cdots)$. Here field theory demands a more critical and more analytical type of thinking. One should distinguish at least two types of problems:

(a) How the perceived psychological situation will look at the time S^4 depends obviously upon whether or not the experimenter will provide food and on similar external physical or social conditions. Everybody will agree, I suppose, that these factors cannot possibly be derived from the psychological field of the individual at the previous time, even if all the psychological laws were known. These factors are alien to psychology.

(b) There remain, however, legitimate psychological questions in this second type of problem. We can keep the boundary conditions of a life space constant or change them in a known way during a certain period and investigate what would happen under those conditions. These problems lie definitely within the domain of psychology. An example is the problem of restructurization of memory traces. We know that these processes depend on the state of the individual during the total period S^{t-n} until S^t (Fig. 2) and are different, for instance, during sleep and while being awake. Doubtless the experiments on conditioned reflex have given us a wealth of material in regard to this type of problem. They will have to be treated finally in the way which we discussed in the beginning, namely, as a sequence of relations between a situation S^t and the immediately following situation S^{t+dt}.

On the whole, I think the psychological trend is definitely going in this direction. For instance, the goal gradient theory has been formulated originally as a relation between behavior and past situations. Straight, analytical thinking demands that such a statement should be broken up into several propositions (12), one of which has to do with the intensity of goal striving as a function of the distance between indi-

vidual and goal. This is identical with a statement about certain force fields and is probably correct. A second proposition implied in the goal gradient theory links the present behavior to the past situation S^{t-n}. The specific form is, to my mind, unsatisfactory. But even if it should be correct, it should be treated as an independent theory. Hull's formulation of a 'Gradient of Reinforcement Hypothesis' is a step in this direction.

V. Psychological Ecology

As an elaboration of our considerations, I would like to discuss some aspects of Brunswik's treatment of the role of statistics (3). I do not expect ever to live down the misunderstandings created by my attack on some ways in which statistics have been used in psychology. I have been always aware that quantitative measurement demands statistics (see Hull's answer to Brunswik [8]). That statement holds also for 'pure cases'; *i.e.*, situations where it is possible to link theory and observable facts in a definite way. Since psychology is increasingly abandoning the inadequate objectives of statistics, further discussion might have little pragmatic value.

However, Brunswik has brought into the open new and important aspects, and I feel that their clarification may be helpful for psychological methodology in general.

Within the realm of facts existing at a given time one can distinguish three areas in which changes are or might be of interest to psychology:

1. The 'life space'; *i.e.*, the person and the psychological environment as it exists for him. We usually have this field in mind if we refer to needs, motivation, mood, goals, anxiety, ideals.

2. A multitude of processes in the physical or social world, which do not affect the life space of the individual at that time.

3. A 'boundary zone' of the life space: certain parts of the physical or social world do affect the state of the life space at that time. The process of perception, for instance, is intimately linked with this boundary zone because what is perceived is partly determined by the physical 'stimuli'; *i.e.*, that part of the physical world which affects the sensory organs at that time. Another process located in the boundary zone is the 'execution' of an action.

Brunswik states correctly (3, p. 266): "The 'field' within which Lewin is able to predict, in the strict sense of the word, is the person in his life space." Then he proceeds, "But the life space is not to be confused with geographic environment of physical stimuli, nor with actually achieved results in the environment. It is post-perceptual, and pre-behavioral." This statement is partly incorrect, namely, insofar as perception and behavior, to my mind, are legitimate problems of psychology. This view is a necessary consequence of the field theoretical approach according to which the boundary conditions of a field are essential characteristics of that field. For instance, processes of perception which should be related to the boundary zone depend partly on the state of the inner part of the psychological field; *i.e.*, upon the character of the person, his motivation, his cognitive structure, his way of perceiving, etc., partly on the 'stimulus distribution' on the retina or other receptors as enforced by physical processes outside the organism. For the same reasons, the problems of physical or social action are legitimate parts of psychology proper.

Brunswik, however, is correct in assuming that I do not consider as a part of the psychological field at a given time those sections of the physical or social world which do not affect the life space of the person at that time. The food that lies behind doors at the end of a maze so that neither smell nor sight can reach it is not a part of the life space of the animal. In case the individual knows that food lies there this *knowledge*, of course, has to be represented in his life space, because this knowledge affects behavior. It is also necessary to take into account the subjective probability with which the individual views the present or future state of affairs because the degree of certainty of expectation also influences his behavior.

The principle of representing within the life space all that affects behavior at that time, but nothing else, prevents the inclusion of physical food which is not perceived. This food cannot possibly influence his behavior at that time under the conditions mentioned. Indeed, the individual will start

his journey if he thinks the food is there even if it is actually not there, and he will not move toward the food which actually is at the end of the maze if he doesn't know it is there.

In the past this principle has not always been adhered to in animal psychology but it seems to me so obvious that I had assumed all psychologists agreed on this point. Statements which could be interpreted otherwise I had regarded as loose terminology rather than an expression of differences of opinion until I listened to Brunswik's paper. The discussion following this paper seems to have brought out the issue still more clearly and it will be appropriate, I hope, to refer to this discussion.

According to Brunswik, it is possible to think in terms of laws rather than mere statistical rules if one limits the psychological field in the way described. However, he claims that for this gain one has to pay "the price of an encapsulation" into a realm of problems which actually leaves out the most dynamic aspects of psychology. He wishes to include in the psychological field those parts of the physical and sociological world which, to my mind, have to be excluded. These parts, he states, have to be studied in a statistical way, and the probability of the occurrence of events calculated.

To my mind, the main issue is what the term 'probability' refers to. Does Brunswik want to study the ideas of the driver of a car about the probability of being killed or does he want to study the accident statistics which tell the 'objective probability' of such an event. If an individual sits in a room trusting that the ceiling will not come down, should only his 'subjective probability' be taken into account for predicting behavior or should we also consider the 'objective probability' of the ceiling's coming down as determined by the engineers. To my mind, only the first has to be taken into account, but to my inquiry, Brunswik answered that he meant also the latter.

I can see why psychology should be interested even in those areas of the physical and social world which are not part of the life space or which do not affect its boundary zone at present. If one wishes to safeguard a child's education

during the next years, if one wishes to predict in what situation an individual will find himself as a result of a certain action, one will have to calculate this future. Obviously, such forecast has to be based partly on statistical considerations about non-psychological data.

Theoretically, we can characterize this task as discovering what part of the physical or social world will determine during a given period the 'boundary zone' of the life space. This task is worth the interest of the psychologists. I would suggest calling it 'psychological ecology.'

Some problems of the 'life history' of an individual have their places here. The boundary conditions of the life space during long- as well as short-time periods depend partly on the action of the individual himself. To this degree they should be linked to the psychological dynamics of the life space. The rest of the calculation has to be done, however, with other than psychological means.

The essence of explaining or predicting any change in a certain area is the linkage of that change with the conditions of the field at that time. This basic principle makes the subjective probability of an event a part of the life space of that individual. But it excludes the objective probability of alien factors that cannot be derived from the life space.

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THE MEANING OF 'MORALE' IN RELATION TO MORALE BUILDING AND MORALE RESEARCH[1]

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I. Id, Ego and Superego Morale

In Professor Allport's memoranda, we find three summary definitions of morale, representative of the views of many psychologists. The first pertains to the individual, the second to the group, and the third to the individual in the group.[2]

Our present national concern with morale in relation to the

[1] This paper attempts a re-view of the problem of morale. A careful and full documentation attempting to collate what is said here with what others have said would add little to its value, but considerably to its length, and has, therefore, not been attempted here. The SPSSI yearbook, 'Civilian Morale,' which was unfortunately not in the writer's hands at the time the present paper was written, is undoubtedly the most important single reference on the morale problem. For a fuller discussion of some of the general concepts underlying this paper, see Chein, I., 'Personality and Typology' to be published in the *Journal of Social Psychology*.

The writer wishes to express his sincere appreciation to Professor Gardner Murphy and to Drs. C. P. Seitz and Max Hertzman for their critical reading of the manuscript, but accepts full responsibility for the content of the paper.

[2] 'Psychological Factors in National Morale,' a memorandum representing one of a series of explorations undertaken by a seminar in Psychological Problems of Morale in the Department of Psychology at Harvard University under the direction of Dr. G. W. Allport and Dr. H. A. Murray. The definitions are: ". . . *morale* refers to a condition of the physical and emotional health in the individual that makes it possible for him to perform his work and live hopefully and effectively, feeling that he shares the basic purposes of the groups of which he is a member; and that makes it possible for him to perform his tasks with energy, enthusiasm, and self-discipline, sustained by a conviction that, in spite of obstacles and conflicts, his personal and social ideals are worth pursuing." "*Morale* refers to the condition of a group where there are clear and fixed group goals . . . that are felt to be important and dominant over individual goals; where there is confidence in the attainment of these goals, and subordinately, confidence in the means of attainment, in the leaders, associates, and finally in oneself; where group actions are integrated and cooperative; and where aggression and hostility are expressed against the forces frustrating the group rather than toward other individuals within the group." ". . . *morale* pertains to all factors in the individual's life that bring about a hopeful and energetic participation on his part so that his efforts enhance the effectiveness of the group in accomplishing the task in hand."

problems raised by the war, make it imperative that our central reference be the individual-in-the-group.

We must not overlook, however, the interrelationships existing among the referents of the three definitions of morale. It is fairly obvious that individual-in-the-group morale presupposes *group* morale, at least to the extent that there are common group goals. We shall return to the problems raised thereby. Of more immediate import, however, is the fact that we may regard it as axiomatic that there is continuous interaction between the morale of the individual and the morale of the individual-in-the-group. We cannot expect that a person whose individual morale is 'low' because of constant pessimistic preoccupation with personal problems of employment and income or family adjustment will furnish hopeful and energetic, to say nothing of effective, participation in the group activity. Conversely, it is highly doubtful that an individual can so isolate himself from a group in which his membership is compulsory that his personal morale will not suffer from poor adjustment to the group. At the same time, we must remember the person who *escapes* his own problems by energetic participation in group activities.

In connection, then, with individual-in-the-group morale, all problems of morale converge. The problem of the measurement of morale would appear to be, formally, simple. One has but to ascertain the answers to the questions that are implicit in the definitions of morale. Thus: "Does the individual participate energetically in the group activity? Does he have faith in the group goals?" Etc.

The problem of morale is, however, more complex. When we focus our attention on the fact that morale is to a large extent a matter of motivation (the definitions are replete with terms signifying this fact: Goals, hope, ideals, etc.), several important distinctions emerge. For not all motives are to be equally valued and, if we do not recognize this fact, it is easy to confuse under the same definition a temporary and unstable quasi-good morale of the individual-*with*-the-group with the genuine and enduring morale of the individual-*in*-the-group.

We propose, in order to clarify this point, to follow the broad classification of motives that is inherent in Freud's distinction of id, ego and superego systems.[3]

We shall speak of id morale where the morale status of the individual is largely determined by id motives, of ego morale where the morale status of the individual is largely determined by ego motives, and of superego morale where the morale is largely determined by superego motives. We shall also subdivide ego morale into the categories narcissistic-ego morale and expanded-ego morale. We shall attempt to show that id, superego, and narcissistic-ego morale, each in its own way, corresponds to what we have spoken of as the "temporary and unstable quasi-good morale of the individual-*with*-the-group" and that expanded-ego morale corresponds to what we have referred to as the "genuine and enduring morale of the individual-*in*-the-group."

Good id morale, in time of war, would correspond to id aggression directed against the enemy, with minimal frustration of other id motives. It is important to distinguish between aggression as an id motive and aggression as an ego motive. In the former case, the goal of injury is an end in itself; in the latter, it is a means to a larger end. The desire to kill the enemy just because it is gratifying to kill the enemy corresponds to an id motive; the desire to kill the enemy because such a procedure is a means of self-preservation or a means of minimizing suffering in the long run, or for both or similar reasons, corresponds to an ego motive.

A difficulty with id morale as a determinant of the morale status of the individual is that the canalization of id motives is so unstable and that displacements so easily occur. So long as the individual's aggression is directed against the enemy, it may be expected to contribute to a hopeful and energetic participation in the group activity which is also directed against the enemy. At the moment, he is *with* the group. But let him be frustrated by the Mayor's sneering remarks at

[3] As the writer has pointed out elsewhere, *loc. cit.*, this classification of motives is of value even though one does not follow, any further, the Freudian instinct theory or classification of motives. For more rigorous definitions of these motivational systems than the present discussion affords, see Chein, I. *loc. cit.*

psychiatrists or the need to pay a large income tax, and his fund of aggression may, as the writer has observed, swing into an entirely new direction and stop pointing at the enemy at all. Or, again, let the enemy be vanquished and the group turn to the problems of reconstruction; he is still occupied with demands for vengeance. Such morale, from a short-sighted view, is good; from a long sighted point of view, exceedingly dangerous. In any case, in evaluating the individual's morale-status, it is important to know how much of his morale derives from this source.

Superego morale is well instanced by Decatur's "My country, may she always be in the right, but my country, right or wrong." This proposition implies a rigid categorical rule of behavior, the support for which by no means obtains in the individual's experience. Superego morale is to be found in such canons of behavior: "To support my country in any action is right, not because I can see favorable consequences to this action, not because I have ample reason to believe that the government is more far seeing than I and is to be trusted, not because my experiences have given me the faith that the people act as a constant check on their representatives and that the people are in the long run to be trusted, but simply because it is right."

Such morale may also, in the immediate situation, be good, but it is likewise not dependable. The cardinal principle on which such morale is based may itself be shattered against other superego principles. These principles are categorical imperatives, not subject, as such, to reason or to rational control. The writer has, for example, observed reactions to reductions in real-estate taxes at the same time that vital social services are curtailed, to discriminations against the Negro in defense and production, to the continuance of certain investigating committees, to alleged characteristics of some of our allies, and so on, in hyper-moralistic terms of indignation. From such a point of view there is often no appeal to countertendencies in the country, to favorable prognostic factors, or even to civic responsibility that must seek ways and means of

correcting evil. "Iniquity is iniquity, there is nothing that can undo it, and the evil-doers must be undone with the evil."

The point is that the superego may be divided against itself, and the opposed tendencies may become activated with different force at different times. Appeals to the superego, and, thereby, the relative strengthening of superego motives in the psychological economy of the individual may yield unanticipated results.

Turning to narcissistic-ego morale, we may choose as a specimen a man who invests heavily in war bonds, because he believes it to be a good investment; who becomes active in defense work, because he thereby makes good business contacts and generally raises his prestige in the community; who rants against subversive elements, but only because it is a good thing to do, because it makes him seem important, and because he has nothing to lose anyhow; and so on. Such morale is also undependable, because the conditions of self-interest are subject to change.

We should not lose sight of the important fact that all three types of morale so far discussed represent motives that are the favorite playthings of demagogues who follow their own interests rather than the public good.

There is no sharp line of demarcation between narcissistic-ego morale and expanded-ego morale. The condition of the expanded-ego is identification of a profound and lasting sort.[4] "His interests are my interests, his needs my needs, his pain my pain." The greater the number of such identifications or the larger the group with whom the identification takes place, the greater the expansion of the ego. The ultimate in such expansion would occur when the individual genuinely succeeds in identifying himself with *the people*. The value of morale of

[4] It need hardly be added that such identification normally leads to a, seemingly paradoxical, but none the less real, clearer definition of the self and is not to be confused with the disruption of the self that is characteristic of some psychotic conditions. A person in love, for example, has a fuller appreciation of the self, not an obscurer one. It is in this light, too, that one can understand the increased resistance that psychoanalysts have found in patients under hypnosis. In topological terms, one might express the phenomenon as follows: as the external boundaries of the ego expand, the boundaries of the inner regions become less permeable and the inner regions become more definitely structured. Pathology of the self ensues when this correlative process is disturbed.

the expanded ego lies precisely in the fact that it avoids the dangers of id and superego morale in that it remains keenly attuned to realities; and it avoids the danger of narcissistic-ego morale in that it guarantees that the needs of the individual remain attuned to the needs of the group. One must not forget, in the evaluation of expanded-ego morale, the necessity of determining whether the identifications upon which the expanded ego is built are with persons or groups that are representative of the people as a whole or with persons or groups that are representative only of limited interests. It is well, also, to be aware of the possibility of identification with the genotypical *man* while experiencing strong antagonism and rivalry to all or some phenotypical men. The stability and dependability of such identification with the genus but differentiation from the species (*e.g.*, the man who is genuinely unhappy about all the death and suffering brought about by the war, but who, in his personal relations, remains predominantly unsympathetic and egotistical) remains a problem for research. It is not necessarily a sham, because, for some people, a necessary condition of 'warmth' and 'understanding' is distance; they are blinded, frightened, or overwhelmed by phenotypic detail. By the same token, the man who violently hates a group, but who finds that all or a significant proportion of the phenotypic specimens of this group of his intimate acquaintance are 'exceptions' represents an interesting problem in social psychology which may have important implications for the problem of morale.

The impression must not obtain that because we have discussed these different types of morale separately, they exist singly in different individuals. The morale of the individual is a complex of id, superego, narcissistic-ego, and expanded-ego morales. What varies from individual to individual is the relative role played by these different factors in his overall morale, the specific motivational content of each of the factors, and, in the case of ego morale, the scope and intelligence revealed in relation to external realities.

Nor must we lose sight of the fact that there is a dynamic interaction between the different factors. For example, a

superego imperative, "In time of national emergency, we must all suffer alike," may have the following effects: "With my country-men dying on field of battle, how can I even think of going to ball games? Or press for a living wage? Or impugn the Mayor's motives when he demands in the name of the war effort that I work an extra day, even though I can't see how my working an extra day will in any way contribute to the war effort?" The ensuing frustration may increase the fund of aggression which may, now, add to the id aggression direction against the enemy; or this aggression may be directed against the 'dying countrymen.' Similarly, the superego imperative "Evil must be punished" may conjoin with id aggression to produce good morale in time of war and bad morale in the post-war reconstruction.

Again, it must be remembered that there is no sharp line of demarcation between id, ego, and superego motives, and that, as a consequence the diagnosis of motives (and morale) from this point of view can never become a mechanical affair. The distinction between id and ego motives rests upon the amount of control invested in these motives (*i.e.*, its lack of the all-or-none character) and on the recognition of the consequences of their gratification with which they are integrated. There are thus two *continua* of variation leading from the id motive to the ego motive: the exercise of control and the perception of reality. The distinction between ego and superego motives rests chiefly on their behavioral support. Motives of ego origin which survive, despite relevant changes in the objective circumstances which justified them, acquire a superego character. It will be remembered that many, if not all, superego motives originate in the ego functions. There is also reason to believe that superego motives exist, so to speak, in layers and that successive layers do not have the same degree of stability and force. Similarly, narcissistic-ego motives which do not involve identification with other persons but which are integrated with the far-sighted view "in the long run, what is good for me, is conditioned by the welfare of others," are essentially akin to what we have called expanded-ego motives. Here, too, there is a dimensional, rather than a categorical, differ-

entiation, a dimension of farsightedness or scope and a dimension of identifications.

Finally, we must remember that while we have pointed to dangers inherent in the mobilization of id, narcissistic-ego, and superego motives, from the point of view of morale, this fact must not be taken to mean that appeal to such motives is never justified. In times of crisis, it may be necessary to resort even to dangerous implements. What is necessary, however, is a good perspective with regard to our morale building objectives, a recognition of the most desirable morale states and a consequent major emphasis in our long-term efforts in these directions, a sober appraisal of the less desirable morale states and a continual effort to offset and to minimize their dangers. It may be necessary to appeal to id motives in morale building, but we must strive to build beside it, and by all the resources at our command, expanded-ego morale, and we must continually strive to direct the id motives into the least harmful channels. Violent and blind id aggression against the top Axis leadership is, for example, probably less dangerous than violent and blind id aggression against the German, Italian, and Japanese peoples, but such complete canalization is itself perilous unless, by its side, is built up an understanding of the conditions which breed such men and of the nature of fascism.

II. What Are the Common Goals?

At many points in the previous discussion, we have avoided an important problem. We have already stated that morale of the individual-in-the-group presupposes morale of the group, at least to the extent of common group goals. It is in terms of common group goals that the morale of the individual must be evaluated. Herein resides a problem which is logically propadeutic to all others, the problem of determining what the common goals are and, more important, the problems of determining the sub-goals which lie in the direction of the ultimate common goals and of determining the criteria in terms of which one can some day say that the goals have been attained.

Superficial examination may indicate that the public opinion poll provides an adequate means of solving this problem.

Closer inspection shows that it does not. Polls today can undoubtedly reveal that the goal of the vast majority of the population is to win the war. But what does winning the war mean? Does it mean the destruction of Hitler, Hirohito and Mussolini? Does it mean the deletion of the *word* 'fascism' from the lexicon of the nations? Does it mean the killing of every last German, Italian and Japanese not now a subject of the United Nations? Does it mean the reduction of the Axis countries to a state of vassalage? Does it mean the abolition of social, political and economic distinctions between different peoples, races, and nations? Does it mean the complete liberation of all colonies and the destruction of imperialism? Does it mean the abolition of the conditions that breed war?

No doubt the public opinion polls can fractionate our people in terms of how they answer these and other questions. But then where are we? Suppose we find that the vast majority of the people have as war aims mutually incompatible goals? Or, suppose we find that the war aims are incompatible with more ultimate aims of our people, such aims as being permitted to live in peace, security and prosperity?

The public opinion poll lends itself readily to the ideals of Baconian science. True, there has been increasing recognition of late that, in the very formulation of the questions, theoretical preconceptions may enter. But such theoretical intrusions are seen as interference. Yet we can see that for purposes of morale research, at least, the results of public opinion research by themselves are meaningless; that they must be integrated and interpreted in terms of some larger evaluative system. The problem of morale is, after all, not far removed from the problem of morality, as a revaluation of what we have said concerning the expanded ego in terms of what Holt has said about ethics in his *The Freudian Wish* would readily show.

A practical necessity in morale research is not merely to know what people want, but also to know what is good for them in terms of what they want.

The problem that herein confronts researchers in morale has much in common with the problem of leadership in a democracy. Shall leaders follow the people they lead, or merely

go along with them? Or shall they *lead?* Is there, perhaps, no place for leaders in a democracy, but only for agents and representatives? If there are to be leaders, they must of necessity plan for and anticipate the needs of their followers. But is not the advocate of a genuine leadership propounding fascist doctrine, the *fuehrer* principle? Do not the fascists tell the people that they must depend on their leaders to determine what is good for them?

The progressive unmasking of fascism that has occurred in recent years has made it crystal clear that the fascists make a mockery of the 'good of the people' and that the fascist leaders, at best, pay only lip service to the principle of seeking the good of the people.

The quintessence of democracy is not that it has no place for leaders, but that it provides checks against the perversion of leadership, through the constant interaction of the leaders and the led, through the interaction of many leaders, through the freedom to choose the leaders to be invested with responsibility, but most of all through the fact that the followers are not blind. The place of leadership in an ideal democracy arises solely from the practical necessity for an efficient division of labor.

The morale researcher must put himself in the place of a leader. He must see near and remote goals in terms of their interrelationships. To speak of good and bad morale, he must plan for the future. In devising his reference plans in terms of which he must evaluate morale, he must on the one hand interact with the people he is investigating (perhaps through opinion polls, perhaps through other devices) and on the other hand interact with other leaders who see the common problems through different eyes, sociologists, economists, representatives of our government and of the governments of our allies, and so on. But plan he must. Nor, devoted as he may be to the ideals of pure science, need he feel ashamed of the terms 'good' and 'bad' as of an anachronism in science. For no matter how much the scientist may seek to keep his values from interfering with his science, he is still a man as well as a scientist; and there are times when his interests as a man and

his interests as a scientist collide. In the problem of morale, the interests of the scientist and the interests of the man do collide.

For the morale researcher and for the morale builder, the first and most pressing need is for a clear statement of war aims. Invaluable as a skeletal framework for such a statement are, of course, the Roosevelt-Churchill Atlantic Charter and the, by now, justly famous speeches of Vice-President Wallace.[5] This skeleton must, however, be given additional substance, made specific and concretized. The more specific the statements of war aims, political, economic, social, the more effectively they can be utilized in guiding our efforts and in appraising our morale resources.

We are well aware that the more specific a war-aim statement, the more unstable, in terms of unforeseen changes in circumstances and improvement in understanding, it is. We can accordingly appreciate the reluctance of our political leaders to issue more specific statements. We can even understand the necessity for such caution, because of the confusion that would ensue for all, save the most advanced and enlightened, of our citizenry with changes in stated war aims. For the leaders, however, clear formulations which can be changed as needs arise are vital.[6]

Armed with such a formulation we can turn our efforts into significant and many faceted research. We can poll, or otherwise investigate, the population for its acceptance or rejection of these war aims, for its participation in their attainment, for its expectations with respect to their attainment. We can through polling and interview techniques ascertain the felt difficulties that cause people to lose hope or to become overly optimistic and we can call upon the psychoanalyst to investigate the more subtle psychological conditioning factors. We can discover the prevalence of information and misinformation that support these attitudes, participations and expectations.

[5] See especially Wallace's speech of May 8, 1942.

[6] A brilliant illustration of specific formulation of war-aims is afforded by Vice-President Wallace's, "America will not have made her contribution until 90 per cent of the adults of the world can read and write, until all of the children of the world can have at least a pint of milk a day."

III. Morale Potentialities

As the concept of morale has emerged thus far, although the development has dealt with motives and values, it is, paradoxically, quite a static concept. For, although we have recognized the hormic and axiological aspects of the morale problem, we have not brought into clear focus the dynamic interrelations between the morale of an individual and the demands and presses which act upon him from without.

It is fairly obvious that morale of the id, superego and narcissistic-ego varieties are dynamically dependent on the demands and presses which act upon the individual, on the opportunities for gratification, on the compulsory sacrifices, on concrete danger situations. The continuities, discontinuities and other variations of such morale states depend to a large extent, as we have already seen, on adventitious circumstances. The 'dynamic' is largely a dynamic of the environment or, at the most, a dynamic of the individual *subjected to* the environment rather than a dynamic of the individual who *exercises control over* the environment. We are, of course, well aware that even an id dominated person reacts selectively to the environment. This selective process, however, is not one which the individual *controls;* it is determined by the momentary states of the individual and the momentary environmental circumstance; it is wholly an impersonal process.

The dynamic of the individual who is not merely a victim of the environment, but who remains the master even while compelled to do its bidding is quite obviously an ego affair. What we have, however, thus far obscured by speaking of expanded-ego morale as 'genuine and enduring' is the fact that even this type of morale is affected by the demands and presses which act upon the individual. For the role of the ego and of the expanded ego is not independent of id, superego and narcissistic ego factors.

Expanded-ego morale is enduring as long as the environmental stresses do not change sufficiently to produce significant changes in the relative balance of the motivational systems that constitute the personality. The impermanence of other types of morale is inherent not only in the fact that they may

fluctuate with changes in environmental stresses, but that they may fluctuate with changes in the environment that do not at all increase the stresses acting on the individual or that even reduce such stresses. Thus, in the case of the id revenge motive or the superego talion law motive or the narcissistic-ego economic gain motive, the winning of the war which immeasurably reduces the stresses acting on the individual may, without any change in the motives or in the balance of the motives, change the values of these motives from good morale factors in time of war to bad morale factors in time of post-war reconstruction.

The significance of impermanence of expanded-ego morale, such as there might be, can still be greatly exaggerated unless we consider it in the light of another concept to which, for lack of a better term, we may refer as *morale potentiality*.

In the ego dominated personality, if we may be permitted the luxury of a truism, there is no yielding in the role of the ego so long as the environmental stresses and strains can be resisted. The capacity for such resistance is, however, an attribute of the ego which may endure through considerable fluctuation in environmental stresses. Such a capacity, if sufficiently highly developed, becomes a permanent attribute of the ego, save under the most extraordinary environmental reverses. It is this capacity which we have in mind when we speak of *morale potentiality*, for it determines the fate of ego morale not only under present circumstances, but under more adverse or under better [7] circumstances. It is this capacity that must be sufficiently diminished before expanded-ego morale can suffer under conditions of great demand or of great adversity. It is this capacity which, to a greater or lesser extent, liberates the *endurance* of expanded-ego morale from the determining influence of the environment and which con-

[7] It is necessary to mention 'better circumstances' because some individuals cannot stand undue 'prosperity' which mobilizes superego needs and thereby changes the balance of forces within the personality. The ability to withstand 'prosperity' introduces a complication in our present argument which is not relevant to the main issue. We shall therefore ignore it here. We content ourselves with a reference to the parable of the Jews who, upon being delivered from Egyptian slavery, had first to suffer the adversities of forty years of wandering in the wilderness before being delivered to the promised land, suffering presumably in order to develop their egos to withstand 'prosperity.'

verts the morale status of the individual into a personality trait.

This capacity to withstand the environment, to maintain one's integrity even while one is swept along in the maelstrom of external forces is the ability of the individual to bear frustration, pain, and anxiety without becoming disrupted, the ability to assume an attitude of 'watchful waiting' without giving up even though he is impotent. In a word, it is the capacity for what the writer likes to call *dynamic resignation.*

Such a capacity, of course, grows under conditions of adversity, provided that these conditions are sufficiently graduated not to overwhelm the individual. There are, however, other factors which contribute to its development and, of these, by far the most important is a sense of perspective.

We can clarify what such a sense of perspective implies by a brief discussion of Maslow's [8] attempt to distinguish between frustration and deprivation. Strictly speaking, deprivation always implies frustration. One is not *deprived* of wings, in any meaningful psychological sense, if one has no desire to fly nor any other need of wings. The distinction that Maslow is seeking is not one between deprivation and frustration, but between the localization of the frustration, on the one hand, and the generalization or diffusion of the frustration, on the other. To illustrate: A man may be deprived of food, but a great deal depends on *what* is being frustrated. What is being frustrated may simply be the appetitive food seeking drive or it may be both the hunger drive and the need for ego status. In the one case, the frustration is localized; in the second, it has become generalized. The same distinction holds for pain and anxiety producing situations. My tooth is being drilled by the dentist. I may react to an intense pain in my tooth, or, as though my entire safety and security is being undermined,

[8] A. H. Maslow, Deprivation, threat and frustration, Psychol. Rev., 1941, 48, 364–366. This discussion is not offered as a criticism of Maslow, but only for its relevance. The major points are thoroughly understood by him; the terminological difference is simply a matter of convention. The use of the expression "generalization of frustration" emphasizes, however, the minor point that frustration may not only be increased by ego involvement, but by *any definition* of a situation by a person, which increases the sheer number of motives that are frustrated.

as though my very life itself is being attacked. My apprehension over a prospective visit to the dentist may be anxious expectation of pain in my tooth, or, anxiety about an assault upon my ego as well.

The meaning of 'a sense of perspective,' we hope, is now clear. It involves the ability to properly localize the source and point of attack of frustration or pain. Strictly, such a sense of perspective is not a matter of the capacity to *bear* frustration, pain or anxiety because it actually serves as a means of reducing frustration, pain and anxiety. It is, however, a way of dealing with *presumptive* frustrating, pain inflicting, and anxiety arousing situations and, therefore, cannot be separated from the capacity to bear these forms of suffering.

In the maturely developed ego, as Allport points out, egocentrism is minimized and, thereby, one of the essential conditions of a proper sense of perspective is already present. That is to say, maturity of the ego is one of the basic dimensions of morale potentiality. As a corollary of the proposition that ego maturity is a function of freedom from egocentrism, we may add that the capacity for ego expansion is an attribute of the maturity of the ego. There is, however, another vital condition of such perspective, namely, an understanding and comprehension of the events transpiring around one, and of the events in which one is involved. Such understanding is another basic dimension of morale potentiality.

It is not always necessary that understanding be complete. The necessity to preserve war secrets is not in contradiction to the necessity to maintain morale. Understanding must, however, be sufficiently developed to make tolerable the effect of ignorance. No scientist, for example, breaks down because he does not know exactly how every one of the scientifically established facts has been established. He does, however, have, as a support, a general understanding of the nature of scientific process. In the same way, a citizen who must be kept in ignorance of a multitude of details may find support in terms of his general understanding of the nature of fascism, the

nature of democracy, the nature of warfare, and the problems of production.[9]

To the extent to which one can diagnose the morale potentiality of the individual, one can evaluate the morale status of the individual not only in the present, but also with respect to situations that he has not yet been forced to meet. It is, moreover, only in terms of such a concept of morale potentiality that one can properly evaluate some of the distinctions in types of morale.

With only the definitions of morale with which we started, a morale dimension in terms of activity-passivity, for example, can hardly have meaning. Passivity is not equivalent to hopeful and energetic participation in activity. At the most, passivity, or failure to participate in the group activity, signifies a readiness to be carried along and an absence of contra-group activity. As such, passivity signifies poor morale. The fundamental dimension of morale, good-bad, would thus itself appear to be a complex dimension having at least three components: degree of active participation in group activity, degree to which the goals of the individual correspond to the goals of the group, and the degree to which the individual expects that the group goals will be attained. Such a system may, however, lead to grave error in the diagnosis of the morale of the individual.

Morale is, as we have already argued, a matter of motivation, whereas activity-passivity is a matter of behavior which includes more than motivation for behavior is conditioned not only by motivation, but environmental circumstance as well. A man may be passive, taking no significant part in the group

[9] There are doubtless other factors which condition morale-potentiality. In a personal communication, Gardner Murphy suggests a factor of 'physiological toughness.' While it is not entirely clear what the psychological counterpart of such 'physiological toughness' might be, there can be little doubt but that, quite apart from the factors we have discussed, people vary in their capacities to 'take it on the chin.' Certainly, the state of physical well-being contributes to the morale potentialities of the individual. The psychosomatic aspect of the morale problem urgently calls for further investigation. The following possibility ought to be considered. The "naive, stupid, non-ego-expanded, but 'tough-gutted' man who makes up the spearhead of many 'shock' and combat organizations" may maintain good individual morale under stress conditions because of self-sufficiency imposed by his own limitations and a quasi-good individual-in-the-group morale because he simply does not know enough to go against the group.

activity because of poor morale, but he may also be passive because he has nothing to do or because he is prevented from doing what he can do. To properly evaluate such conditions, the temporal dimension which is provided by *morale potentiality* is essential.

Similarly, with the dimension defensive-offensive morale. This dimension emphasizes an important issue which has been hotly debated throughout the United Nations. The immediate strategic war needs should not, however, obscure the fact that there are times for defense as well as for offense. A defensive spirit when offense is indicated is a condition of bad morale, but so is an offensive spirit when defense is indicated. Again we meet the need for a temporal dimension of morale such as is provided by the concept of morale potentiality.

Some individuals give themselves over to a thousand and one more or less coordinated activities that are more or less relevant to the war effort. Others are continually occupied with problems of post-war reconstruction to a degree that interferes with the adequacy of their coping with immediate problems. Others give a disproportionate attention to what are now lesser problems, at the cost of under-emphasizing the more pressing problems; and still others ignore the lesser problems entirely.

Such considerations make evident the need of a 'synergy' dimension in evaluating morale-relevant activities and needs with respect to the morale of the individual and of the group, but here again we must recognize the situation that confronts the individual and, again, the basic coordinating concept of morale potentiality becomes quite relevant.

Some individuals dissipate their energies because of a 'constitutional' incapacity for sustained and concentrated effort. Short of providing such individuals with psychoanalyses, one may effectively prevent the onset of demoralization in these persons by providing them with short term and variegated tasks. Other persons may be dissipating their energies because of the lack of sufficient personnel for the tasks at hand, and the remedy is to provide the personnel. The decision as to the necessary remedy, however, must always be

made in the light of the morale potentialities of the individual in different situations.

Finally, with respect to the hopeful-expectancy dimension, various diagnoses are necessary in order to achieve an adequate understanding of the morale status of the individual. Is the hopefulness blindly autistic, guided only by an optimistic, happy-go-lucky, living-in-the-moment temperament, or is it dictated by a clear-sighted vision of the effective measures that must be undertaken? Is the pessimistic outlook dictated by an objective appraisal of difficulties, or, by a blind distrust of the powers that be? Does the pessimism that rests on an objective appraisal of difficulties otherwise demoralize the individual, or, does he continue to fight and to take the most effective measures that are available to him? Such diagnoses lead not merely to an appraisal of the individual's morale in the present, but to an appraisal of his morale potentialities for future situations.

IV. Conclusion

The above considerations may serve as a guide to morale building and to morale research programs. They direct attention to the qualitative as well as quantitative aspects of the morale building and morale maintenance programs. They demand critical appraisal of these programs in terms of expressed objectives of such programs. They direct attention not merely to the individual, but also to the environment; not merely to the present and immediate future, but to the relatively distant future as well; not merely to the irrational and non-rational factors in morale, but even more to the rational factors; not merely to the objective of building morale, but to the objective of developing a mature and well-rounded citizenry, an objective which, in a democratic society, is inseparably intertwined with the objective of raising the level of morale. They point not merely to positive values, but to dangers as well.

One can stop here, and in terms of one's generic and intuitive understanding of personality, of id, ego, and superego factors, of war and post-war aims, plan projects, choose

critically among alternative projects. One can, for example, with respect to a particular project, estimate, as well as one is able, whether it will stimulate id aggression or ego aggression against the enemy and, if id aggression, whether it can be controlled by effectively integrating it with ego aggression or, when needs be, effectively diminished.

Time is pressing and we cannot afford to wait until we are certain about every step. We must muddle along as best we know how, and we cannot afford to sneer at the 'best,' no matter how manifestly inadequate it may be. Yet while we are muddling along, we must from every possible direction, and continuously, strive to improve this 'best.'

It is in the course of this continuous striving to improve the 'best,' and in terms of it, that we must coordinate our research. To get the most out of this research, we must not oversimplify the general character of the morale problem as so many morale researchers appear to have done and we must recognize the nature of the insufficiency where a given study is directed to only a *part*-problem of morale. It is in the hope of contributing to a clarification of the complexity of the full problem of morale that this paper was written. What we have attempted here is to build a conceptual framework in terms of which the morale problem may be grasped. Too many morale-workers, for example, seem to feel that their task has been accomplished when they have ascertained how many Americans trust and how many distrust our allies, the leadership of our armies, and so on. Such information is doubtless of value. In terms of the kind of framework we have constructed, however, we can see far more clearly the limitations of this value. The mere cataloging of attitudes deemed relevant to morale without setting these attitudes in their proper individual motivational and cognitive context is insufficient as an end in itself, or as a means of predicting morale-dependent behavior of the individual, or even as a preliminary step in studying the conditions of morale.

IS THE NEWBORN INFANT'S REPERTOIRE LEARNED OR INSTINCTIVE?

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Common sense sees a large instinctive element in the behavior of the newborn infant, since the opportunities for learning in utero are obviously limited. However, from time to time, it has been indicated that the possibility of learning is not entirely absent prior to birth, and some writers have gone so far as to suggest that the neonate's repertoire in its entirety may have been learned during the fetal period.

Among the most outstanding of the latter group is Holt. In *Animal Drive and the Learning Process* (**9**) he very cleverly suggests mechanisms whereby responses, even reflexes, might be learned during the period of gestation. His treatment of the grasp reflex is the most striking case. He notes that whenever the muscles of the hand are flexed, from whatever cause, the skin of the palm is stimulated. He argues that after several self-stimulations of the palm have occurred in association with hand closure, stimulation of the palm becomes sufficient to elicit the grasp reflex. The same theory has been presented by Givler (**6**) and Hollingworth (**8**).

Such a theory, of course, makes use of the easiest instance. Had the congenital stimulus for the grasp reflex been contact with the *back* of the hand or with the forearm instead of stimulation of the palm, the task of the theorist who wished to account for the neonatal stimulus in terms of learning would not have been so easy. When reflexes other than the grasp reflex are examined, serious difficulties are encountered with an interpretation based upon fetal conditioning.

It will be instructive to consider the possibility of learning the knee jerk. Here the theory which accounted for the grasp reflex fails to apply. Post-natally the adequate stimulus is a blow applied quickly and forcibly above or below the

patella. According to Holt's theory, movement of the leg ought to be the stimulus to the knee jerk, since the knee jerk stimulates proprioceptors. Actually, movement of the leg does not cause a knee jerk.

Similarly, if the knee jerk were elicited in utero, another consequence would be that the propulsion of the first toe against the enclosing membranes. In this instance, according to Holt's theory, the neonatal stimulus to the knee jerk should be not a blow applied below the knee cap but a blow applied to the end of the big toe. A sweeping application of the conditioned reflex theory to the problem of fetal development must take into account the fact that some stimuli do not bring the results which would be expected on the basis of the learning hypothesis. One is forced to conclude that the grasp reflex is merely an instance in which the facts accidentally fit the learning hypothesis. If the hypothesis were correct, all other instances should fit it equally well, and this they do not do.

The greatest weakness of the theory of development through learning lies in the fact that in regard to most aspects of neonatal behavior the possibilities of intra-uterine learning do not seem to exist. This can be illustrated with regard to several classes of responses. Among the response mechanisms which are best coordinated at birth are the visuo-ocular responses. In the newborn the pupillary response is present, and it is consensual to monocular stimulation. Eyelid response also is present. When the newborn infant is awake in dim or in moderate light the lids are open; they are closed in response to intense illumination. These eye-lid responses also are coordinated. In some newborn infants visual fixation and good degree of visual pursuit are present; in others, the responses are present but are imperfect. In practically all infants the movements of the two eyes are coordinated (4).

The responses just described indicate briefly the extent of the congenital visual-motor coordinations. Now there are no reasons to believe that light stimulation occurs prenatally. How can a learning theory account for the existence at birth of complicated reactions to visual stimuli, if light stimulation has never taken place prior to birth?

Such questions could be multiplied many times. To cite further instances, the following problems may be proposed: What is the possibility of learning to cry before breathing takes place? How could crying have been associated, through experience, to the numerous stimuli which are encountered only after birth and which instigate crying upon their first presentation, such as heat, cold, intense tastes, electric shock?

Can sneezing occur during the gestation period? The Moro reflex? If they cannot, how can these motor patterns or their stimulus-response connections be learned? Some infants at birth can balance the head when held upright. In the case of this reaction, as in the case of many others, the restraint of the fetal state renders the prenatal practice of such a response impossible.

It cannot be denied that there *are* responses to which a intra-uterine learning theory seems applicable. The grasp reflex does not stand alone. Many responses are elicitable before birth, and it is possible to urge that some response patterns and their stimulating conditions may be affected by these prenatal elicitations. Among such responses are sucking, swallowing, turning the head, moving the limbs. Let us now examine the question as to whether the performance of these responses before birth leads to the formation of any conditioned responses.

In this connection studies by Marquis (15, 16), Wenger (24) and Wickens and Wickens (25) and others (11, 12) on the possibility of conditioning the neonate immediately come to mind. We need to ask whether or not experiments on neonatal conditioning have any bearing upon the question of fetal behavioral development. We think they do not. The possibility of early post-natal training should not be confused with the question as to whether or not conditioned responses are formed under the normal uterine conditions which precede birth. The experiments on conditioning in the neonate necessarily deal with artificial conditions, whose similarity to intrauterine conditions is problematical.

Classical conditioning, as an experimental procedure, consists of a training sequence whereby new stimuli are made

effective. Even if this result could readily be attained in the fetus, it is doubtful that the process could account for an appreciable part of infant behavior. Conditioning, defined in the terms outlined above, is not known to be able to account for *new patterns* of behavior. It can be called into play as an explanatory concept only in regard to the establishment of the stimulus control of responses, not for the creation of new modes of behavior. It is true that according to some theories the formation of response patterns may be accounted for by the laws of the conditioned reflex. These views, however, are theories which are not as yet verified, and they do not offer a secure basis for the learning hypothesis of fetal development.

It is important to note that operant conditioning and trial-and-error learning have not yet been demonstrated either in the newborn infant or in the fetus.

It is difficult to perform experiments in human learning prior to birth. For this reason, the study of early learning has been, and probably will be, concentrated largely in the neonatal period. There is no desire on our part to deprecate such research. The remarks which we have made above are not a general criticism of experiments upon learning in the neonate; they are simply comments concerning the relevance of such studies to the question of the development of fetal behavior. It is recognized that not many experimenters would propose that researches upon learning in the newborn are relevant to the topic of the growth of fetal behavior. Nevertheless it needs to be emphasized that the demonstration of learning in the newborn cannot establish the view that learning constitutes the basis for the prenatal growth of behavior.

Many human responses are capable of elicitation not only at birth, but long before the normal end of gestation. The infant born after eight months' gestation is not lacking in any of the responses present in the infant of normal term, although the responses of the infant which is one month premature may be slower, and less forceful, and less well coordinated. Practically the same thing may be said of the behavior of the child born at seven months. The nursing reflex

may be weak, but it is present. All of the usual forms of neonatal behavior may be elicited (19). It is obvious, therefore, that the fundamental developmental processes take place long before birth. In other words, the formation of conditioned responses at birth does not show that what is elicitable at birth has been learned, since most of the congenital responses are demonstrable at a period which antedates by at least two months the period of birth.

Indeed, in order to explain the behavioral development of the fetus in terms of learning, the fetus would have to be proved to be capable of learning long before the seven-month's stage is reached. This is shown by a survey of the behavioral development of the fetus, which reveals the extent to which behavior is present in the fetus at various ages. Below is given a brief statement concerning the fetal ages at which the various patterns may first be elicited. In this account we have borrowed freely from Munn's (18) excellent summary of the work of Minkowski (17), Bolaffio and Artom (2) and Hooker (10).

There is clear evidence of neurogenic movements in the third fetal month. Among the responses observed in this month are the patellar reflex, a slight grasp reflex, the suggested beginnings of the sucking reflex, and toe responses similar to the Babinski. In the fourth month these responses become more precise and their areas of stimulation become somewhat more specific. A similar increase in definiteness of pattern occurs during the fifth month. In addition, in this month are added the abdominal reflex, various labyrinthine reflexes, the 'trot' reflex and Ahlfeld's 'breathing movements.' In the sixth month some infants prematurely born cry and breathe, and may survive for some hours. This cursory review of fetal development shows at what an early period some behavior originates, and reveals to what extent a learning hypothesis of behavioral development must be projected into the prenatal period, if it is to account for all congenital responses.

Attention must be directed also to the neurological condition of the fetus and the neonate. It has long been known that the behavior of the neonate is relatively normal even

when the entire cerebrum is absent. Dating at least as early as the beginning of the seventeenth century, there have been numerous reports of monstrous human births congenitally lacking the cerebrum, or lacking even a larger portion of the brain. While these anencephalies are usually stillborn or very short-lived, they may live for several minutes or even for several hours, during which time their behavior may be observed. In referring to the behavior of acephalies, it must be kept in mind that few of the observers were especially interested in behavior and for this reason the account of the responses of each acephaly is probably incomplete. Nevertheless, it is worthwhile to examine these records. The acephaly observed by Lallemand (13), cried, breathed, sucked and gave the grasp reflex. Spezza's case (22) breathed, cried and moved its limbs. Putnam's account (21) of an acephaly shows that it breathed well, cried, sucked, swallowed; there were occasional movements of the limbs and of the eyelids. This creature possessed no nervous structures above the medulla. Preyer (20) described an acephalous child, entirely without a cerebrum, which cried, sucked, swallowed, grasped, yawned, thrust out its tongue, turned its head from side to side and sneezed. The child reported by Angell and Elsner (1) "appeared able to direct its gaze to a limited extent, a lighted candle holding its attention." It gave a slight pupillary reflex.

If all of the neonatal responses now known (4) had been tested in these acephalies, it is likely that most of them could have been elicited. As it is, it is clear from these cases that many response patterns of the newborn need not involve the cerebrum, a conclusion which has been drawn from data such as those cited above by many writers including Marshall Hall (7) and Preyer (20). Experimental procedures by Minkowski (17) and Bolaffio and Artom (2) likewise have revealed that the cerebrum exercises little or no control over fetal behavior.

These facts present another serious obstacle to a learning hypothesis of fetal behavioral development. It is true that recent experiments (3) have shown, contrary to Pavlov's claim, that conditioning can occur in decerebrate preparations.

Nevertheless, such conditioning is difficult to obtain, and it differs in several respects from conditioning involving the cortex. The cortex is certainly the structure whose presence makes learning most easy. To suppose that an organism in which the cortex is practically non-functional develops behavior through learning rather than through neural growth means the acceptance of a difficult hypothesis for which there is no supporting evidence.

It was mentioned earlier that acephalies ordinarily live but a few hours or a few days. Occasionally, however, they do live much longer. Most outstanding in this connection is the case, reported by Gamper (5), which lived two and one-half years. During this period bodily growth was fairly normal, and the child was in good health, *but no behavioral advance took place.* This would seem to be a crucial case for the learning theory. If prenatal development occurs by virtue of a sort of subcortical learning, one wonders why such learning did not continue to occur in this acephalous infant. On the other hand, if behavior develops through neural growth, as the maturation theory holds, it would not be expected that this child would show any postnatal behavioral development since there is no reason to believe that any neural growth took place during its postnatal existence.

In further support of this view is the case reported by Talbot (23), an acephaly which lived for eight months without exhibiting any behavioral development.

The anti-instinct doctrine has held sway in human psychology for twenty years, just as the instinct doctrine held sway before it. The time has been reached, we believe, to cease taking our instincts in an all-or-none manner, and to examine the evidence for the learned or unlearned nature of each bit of human behavior. That fetal responses are learned is a hypothesis which should be carefully examined. Our examination of the question leads us to believe, despite the ingenious suggestions of Holt (9) and Hollingworth (8), that most of the behavior of the infant at birth is unlearned, and that fetal development is almost entirely a matter of maturation.

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THE CONCEPT OF NORMALITY IN CLINICAL PSYCHOLOGY

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I. An Examination of Current Concepts

Prevailing psychiatric usage identifies the term 'normal' with the social reaction-average (3). Broadly, the latter may be alternately defined in terms of 'adjustment' or, more cautiously, in terms of the statistical mean, median, or mode (plus probable error).

The Hinsie and Shatzky *Psychiatric Dictionary*, utilizing the first alternative, has this to say about normality: "From the psychiatric point of view, a psychically normal person is one who is in harmony with himself and with his environment. He conforms with the cultural requirements or injunctions of his community. He may possess organic deviation or disease, but as long as this does not impair his reasoning, judgment, intellectual capacity and ability to make harmonious personal and social adaptation he may be regarded as psychically sound or normal (8, p. 372)." The attitude of Kraines is similar: "Normalcy consists of a type of adjustment which often, though by no means always, makes for a minimum of discord between the person and society. Members of society in the modal range (*i.e.*, between the extremes) have managed to work out a fairly 'livable' adjustment (10, p. 259)." Glover states the same position in a somewhat paradoxical fashion: ". . . normality may be a form of madness which goes unrecognized because it happens to be a good adaptation to reality (5, p. 165)."

The more cautious alternative is structurally more formal. Normality is 'operationally' defined by applying one of the measurements of central tendency to a 'normally' distributed

population.[1] The relation of the 'adjustment' concept to the statistical concept is transitive, *i.e.*, the first implies the second, although the converse is not true.[2]

Thus, it is expected on formal grounds that, although the two definitions are intensionally distinct, they may be identical in extension.[3] The doubtfulness of any uniform *verification* of this last assumed dichotomy has been experimentally demonstrated by Edwards (4), who further recommends a division of vocabularies whereby the psychologist will use the term 'average' (= mean) and the clinician will use the term 'normal' (= adjustment). Although a divisive arrangement of this sort appears to create more problems than it solves, we shall not be able to quarrel with it here. Instead, in the following discussion, the term 'normal' will be taken broadly to mean 'adjustment' (where the statistical concept may indifferently occur as a subsidiary element).[4]

II. Criticism of Current Concepts

In so brief a paper it is impossible to elaborate all of the objections to this position. Consequently, we can offer only the following skeletal list:

1. *The Self-imposed Isolation of Psychiatry.*—In accepting a relativistic norm, psychiatry isolates itself from its culture. It is only superficially paradoxical that a discipline oriented around a social reaction-average must alienate itself from putative subject matter. Strictly, the clinician must find all cultures incommensurate, and the population of his own culture, regardless of its 'values,'[5] a normal standard grey. Moral and aesthetic norms may as well reside in another

[1] Edwards (4) has assembled a number of these statistical concepts. The statistical concept will of course depend on the prior definition by the psychiatrist of the continuum along which the modal (or other) range is determined.

[2] The 'adjustment' concept is ostensibly a more complete description of the 'normal' man and includes the statistical notion. *I.e.*, the statistical concept will not describe the actual adjustment mechanisms which operate.

[3] Where intension means definition by the prescription of properties which must be exhibited, and extension means the total class of entities exhibiting the intension.

[4] *I.e.*, the discussion will dwell specifically on the 'adjustment' thesis without emphasizing the statistical concept, which may or may not be present.

[5] The psychiatrist cannot, of course, offer any non-relativistic or non-reductionist account of value.

galaxy, for they are provided no room in the psychiatric universe, except in so far as their professors constitute the clinical subject-matter (7).

The foundation of the difficulty is this: *The psychiatrist's norm is wholly inadequate as a means for achieving a unified intellectual attitude.* If the clinician seriously accepts the implications of his norm, man is unduly compartmentalized. More specifically, no physician would seriously assert that a man is healthy simply because he conforms to the specifications of a social reaction-average. The implications are clear: In accepting an 'adjustment' concept of normality, the clinician is required to regard 'mental health' as something distinct from 'physical health' and thus, implicitly, to perpetuate the mind-body dichotomy, the hypostatization of 'mind,' and all they stand for.

2. *Internal Difficulties in the Formulation of a Psychiatric Norm.*—In some cases it is possible to find 'abnormalities' which occur on a mass basis. The obsessive behavior of Puritan witchcraft and witch-burning pervaded a whole culture. Presumably, according to the psychiatric position, the 'abnormality' disappeared simultaneously with its general adoption by the community. To explain this sort of transition consistently and wholly requires a set of basic postulates more flexible and fruitful than those of the psychiatrist. Let us examine this problem in another way.

For a discipline which is rightfully proud of dynamic properties, psychiatry is filled with paradoxical surprises, not a few of which derive from a persistent strain of staticity.[6] The psychoanalytic doctrine furnishes us with an example. Glover (5) describes the "normal psychoses and normal neuroses of childhood" as occurring almost universally among children. This would seem to mean any of the following things: (a) that psychoses are not necessarily disease-entities; (b) that, once established as *universally present* syndromes, 'psychoses' are no longer psychoses; or (c) that 'normal' and 'abnormal' do not in fact have *any* consistent meanings. This deep ambiguity is never satisfactorily cleared up.

[6] For a description of the staticity of one kind of adjustment theory, see Sarbin (11).

Let us return to the instance of the community adoption of witchcraft, in order to look at the semantic difficulties involved in the transition from individual 'abnormality' to what is ostensibly group 'normality.' Assume that the individual 'abnormality' is somehow dissipated as it is gradually acquired by the community. If this is true, any assertion made about this transition would be, not an empirical one (*i.e.*, not synthetic), but tautologous (*i.e.*, the most primitive form of analytic proposition). The sentences which presume to describe the transition will be sentences *about* the psychiatric language and not about the sequence of events which constitute the transition.

From a slightly different point of view, the psychiatric position as stated in the last paragraph is analogous to pleasure-pain analysis, which is similarly concerned with *ex post facto* statements about language.[7] From this perspective 'abnormality' becomes, not a specific deviation from phylobiological requirements, but almost anything which obtains societally.

The internal difficulties underlying this general orientation are illustrated in a recent study of the formation of group norms (9). The experimenter administered personality tests (with rating scales) to small internally cohesive student groups, the members of which evaluated on a number of items both themselves and each of the remaining members of the group. A subject who rated himself significantly higher than the group-rating for him, was defined as 'maladjusted' by the group.

This much is clear and consistent enough. The danger appears when it is assumed that not only does the group believe the deviate individual to be 'maladjusted,' but that he actually is 'maladjusted' or 'abnormal.' The objection to this latter conclusion is that the social repercussions of the individual judgments have been ignored. For example, if subject A is accounted 'abnormal' (by the experimenter) as the result of a rating-differential, then subject A's ratings of subjects B, C, D, etc., must be successively adjusted to com-

[7] Statements about language are, of course, dangerous only when they presume to be more than this, *i.e.*, when they set out to describe events external to the language.

pensate for the impact of those factors of A's self-rating which hold equally for A's ratings of B, C, D, etc. More generally, every term in a given matrix must be adjusted to the new relationships which obtain after the manipulation of any unit in the matrix. Since the number of units in the matrix is finite and since appeal to an extra-systemic norm is denied at the outset, we are confronted with an infinite regress, *i.e.*, internal readjustment of terms in the system must be carried on endlessly.

3. *The Lack of Validation for the Psychiatric Norm.*—Assuming that abnormalities may in fact occur on a mass basis, it is quite possible that, by intra-specific competition, the 'normal' man may destroy himself.[8] There is nothing in the fundamental psychiatric position which would prevent racial suicide from being perfectly 'normal.' This consideration exhibits the crucial lack of formal justification for the psychiatric norm. *Validation of any fundamental concept*[9] *demands sufficient provision for the persistence of its subject-matter*, in this case man as phylum.

In the last section of this paper, we shall suggest the nature of the kind of norm which can adequately justify itself.

4. *A Concluding Objection and a Transition.*—Many cultures are for one reason or another so manifestly 'valueless' and 'unwholesome' that, in spite of second-order repercussive effects, the sensitive individual who rebels from the prevailing *mores* and so achieves a certain 'psychic distance' from his culture, is in many respects more 'adequate' and 'normal' than his conformist contemporaries. It is urged that a new concept of normality be formulated: one that will do justice to the 'adequate' responses of dissenting individuals.

This argument may at first glance appear to suggest no more than a verbal transformation, since the above-mentioned culture is 'unwholesome' and 'valueless' only in so far as it is distinctly deviate from the still unarticulated formally justified

[8] *I.e.*, competition among the members of a species weakens the viability of the species. See J. B. S. Haldane (6).

[9] In a material system (or, somewhat loosely, in a system of synthetic propositions). Sentences in the material mode, the terms of which have no referents, become pseudo-sentences.

norm. What is especially emphasized is that a transformation of perspective will permit us to offer a more coherent, systematic, un-isolated, and formally validated account of one important aspect of human behavior.

III. The Phylobiological Norm

Dr. Trigant Burrow (1, 2, 3) and his associates have suggested the adoption of an objective norm based on phylobiological requirements. This sort of norm is open to none of the objections raised in the previous section against those which depend on a social reaction-average. Further, a phylobiological norm affords the possibility of specifying the conditions under which the maximum phyletic homeostasis will obtain. "It is the phyloanalytic position that no behavior-adaptation within the single individual can be healthy and complete in the absence of a healthy basis of behavior adjustment within the community as a whole. The therapeutic attempt, therefore, is not merely to heal the individual but to develop a healthy community basis of behavior that will be effective throughout the organization of man as a phylum" (1, 413–414).

Thus, instead of merely 'adjusting' a non-conforming individual to any culture he happens to be in, there will be a shift in emphasis to the revision of the culture as a whole. This statement does *not* imply that the psychiatrist will degenerate into a moralist, but that a careful analysis of *inadequate* responses will enable the scientist to suggest cultural (*i.e.*, social, political, and economic) modifications, which will be experimentally validated or invalidated by the process of history.

It is confessed that, at present, it is impossible to present any precise bill of specifications. This difficulty, however, is not insuperable, since the norm provides a conceptual orientation [10] which by specific suggested researches [11] can

[10] That this perspective is practicable and experimentally fruitful is indicated by some of Burrow's work (2, 3).

[11] Conducted preferably by the collective action of sociologists, psychologists, pathologists, etc. It is a virtue of this kind of norm that it conforms with progressive work in other fields, *viz.*, with instrumentalist philosophy, the work of Lynd and others in the social sciences, and the unified science movement.

eventually further describe the optimal societal conditions for collective adjustment. It is thus possible to characterize the mean population of a whole culture as abnormal in a meaningful sense, and hence to define a system by appealing to a norm which is not entirely included in it (the system), *i.e.*, by referring each culture to the history of culture. It is the only manner that the fundamental orientation of psychiatry can be adequately arranged.

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EDWIN BURKET TWITMYER

1873–1943

Edwin Burket Twitmyer, Professor of Psychology and Director of the Psychological Laboratory and Clinic at the University of Pennsylvania, was born in McElhatten, Pa., on September 14, 1873, and died in Philadelphia, after a brief illness, on March 3, 1943, in his 70th year.

After receiving a public school education, he attended Lafayette College, receiving the degree of Ph.B. in 1896. The next year he was appointed Instructor of Psychology at the University of Pennsylvania, and remained continuously a member of the faculty of that institution for nearly 46 years until his death. Twitmyer received his Ph.D. from the University of Pennsylvania in 1902. In 1933, Lafayette awarded him the degree of LL.D. honoring one of their outstanding alumni. He was made Assistant Professor at Pennsylvania in 1907; Professor of Psychology in 1914 and, since that date was Director of the Psychological Laboratory and Clinic and more recently Chief of the special Speech Clinic.

The year before Twitmyer came to Pennsylvania, Lightner Witmer had established the Psychological Clinic (1896) and the rapid development of this field demanded Witmer's time and thought and energy to a greater and greater extent. Hence Twitmyer's early academic years were concerned primarily with experimental problems. He showed great ingenuity in the development of apparatus and this interest was maintained throughout his life.

Twitmyer's doctoral dissertation was in the experimental field. The title is *A Study of the Knee Jerk* published in 1902 in Philadelphia by John C. Winston. This work proposed to study variability in the normal and in the augmented patellar tendon reflex. Two forms of augmentation consisted of the ringing of a bell just before stimulation while the subject sat in a relaxed position and also vigorously squeezing the hands

on the bell signal. The stimulating hammers were released from electromagnets when the circuit was broken by a rather elaborate method (for 1902 at least) of procuring constancy of stimulation. One day something went wrong with the actuating mechanism so that the bell rang but the stimulating hammers failed to fall. It seems worth while to describe this accident in the actual words of Twitmyer's thesis (pp. 24–25).

> During the adjustment of the apparatus for an earlier group of experiments with one subject (Subject A) a decided kick of both legs was observed to follow a tap of the signal bell occurring without the usual blow of the hammers on the tendons.

Here then was the first experimental demonstration of a conditioned reflex in man. In interpreting this chance and accidental result, Twitmyer said further in his paper:

> Two alternatives presented themselves. Either (1) the subject was in error in his introspective observation and had voluntarily moved his legs, or (2) the true knee jerk (or a movement resembling it in appearance) had been produced by a stimulus other than the usual one.

Twitmyer immediately changed the emphasis of his experimental work to investigate this curious new phenomena. He discovered that he could elicit it in the case of each of his six subjects. He determined that it required from 150 to 238 combined stimulations by bell and hammer to establish the phenomenon. That the stability of the response alone was a function of the number of combined stimulations. That his subjects all reported not only that they were not kicking voluntarily but even that they were unable to inhibit the response no matter how they tried. That he could determine no difference between the qualitative form of the curve of the normal knee jerk and that elicited without actual stimulation of the patellar tendon. In this connection, Twitmyer pointed out that his apparatus was not adequate to completely verify this matter of qualitative form of the response and that a graphic recording apparatus of much greater speed would be required. And incidentally, this is an experiment which has never been done, although its importance cannot well be over estimated.

There seems some evidence that Pavlov and Bekhterev also hit upon the conditioned reflex by accident and, also like Twitmyer, realized its importance. It happened that Pavlov and Bekhterev made this accidental observation while working on animals while Twitmyer made the similar observation on human subjects. This early work of Bekhterev and Pavlov was unknown, or certainly not widely known in America at the time of the Twitmyer experiment in 1902, and their work was certainly unknown to Twitmyer until a number of years later.

It seems incredible that a finding such as Twitmyer's should have remained unnoticed by other psychologists in America. Twitmyer himself, in his thesis, promised additional experiments which never materialized. The reasons for this failure are complex. Twitmyer's actual thesis, although dated the year of his doctorate, was not actually published until some years later and, being privately printed, received a very narrow distribution. Hence the only immediate publication of the results was an abstract of a paper read before the American Psychological Association at the Christmas meeting in 1904 which appeared in the *Psychological Bulletin* 1905, Vol. 2, 43–44 and entitled 'Knee jerks without stimulation of the patellar tendon.' This abstract clearly sets forth the findings outlined above but even its provocative title seemed to have aroused no interest at the moment and, it would appear, future generations of psychologists do not read old abstracts of papers read at the meetings of the Association.

The story of the reception of this paper at the meeting has been told the writer many times by Twitmyer. William James was Chairman of this session. Inasmuch as he was one of the younger psychologists present, Twitmyer's paper had been placed at the end of the morning program. There had been considerable discussion of several of the previous papers and the time for lunch had passed. Twitmyer was given courteous attention during the reading of his paper. Immediately after the reading was completed, James said (in effect at least), "This is another interesting example of learning. If there are no comments, I shall declare this

session adjourned." Naturally this stifled all discussion, and had there been fruitful discussion, one may be sure that either Twitmyer or some other investigator would have continued research in the development of these concepts. But Twitmyer came away discouraged, with dampened enthusiasm and lack of motivation to continue experimental work on the problem.

Still another factor which led Twitmyer away from the experimental field was the growing importance of the Psychological Clinic at Pennsylvania. One of Witmer's early cases involved defective speech. Mary Marvin Twitmyer, whom Twitmyer married in 1907, was a teacher of the deaf who was brought into the Clinic at an early date. It was from her that Twitmyer developed an interest in the field of diagnosis and training in defective speech. This interest culminated in the establishment, in 1914, of a special Speech Clinic, with Twitmyer as Director. This field of endeavor claimed more and more of his interest, his time and his energies as time went on.

His point of view toward the problem of speech defects—their analysis and correction, will be found in the book, written in collaboration with Y. S. Nathanson and entitled *Correction of Defective Speech*.[1] In this text he emphasizes the fact that disturbances of the normal rhythm of breathing constitutes the nearest approach to a common etiological factor among the great variety of organic and psychological correlates of speech disorders. From the corrective aspect, Twitmyer found that these disturbances of normal breathing were, in part, habitual in origin and that treatment might consist in the breaking up of these bad breathing habits and in the establishment of new and correct ones.

The bibliography of Twitmyer's scientific publications is not large. This is primarily because of many other activities, particularly during the later years of his life. He was the psychological representative on the Board of Trustees of a number of institutions caring for children. He took over the entire burden of the executive head of the Department of Psychology at Pennsylvania for almost thirty years and, under

[1] Philadelphia: Blakiston, 1932.

his wise control, the Department saw an enormous expansion in teaching staff, student body, academic offerings, clinical service and research. He gave considerable time to a private clinical practice. He was an accomplished popular lecturer and did much to popularize both psychology as a science and its application by the clinical approach through this medium. He served continually on many general University committees and contributed largely to the formulation of general University policy.

In a certain sense, Twitmyer's death may be considered in the nature of a war casualty. With his retirement from academic life not far distant, Twitmyer labored more continuously than ever during the last eighteen months of his life. He assumed some of the teaching burden of various members of his staff who were called into active service or who were otherwise contributing to the war effort. He spent long hours and was forced to forego any vacation last summer in order to establish the psychological testing of inductees at the Philadelphia Induction Center until this service could be taken over by Army personnel. As a result his terminal illness found him so fatigued and with such lowered resistance that recovery was improbable at any time. It can well be believed that Twitmyer would have preferred to end his life as he did in harness and in active psychological service.

One cannot end a necrology of Edwin Burket Twitmyer without mention of his humanity—the outstanding characteristic of his personality. Twitmyer liked people. He made their problems his problems. He considered the members of his staff as his children and was interested in their personal as well as their academic welfare and their problems. An apparent gruffness in manner was merely overcompensation for one of the kindest and softest of hearts. There are few of us, who have had contact with Twitmyer, who cannot remember many occasions when he has given sound and sensible advice and when he has helped us through trying times. It is therefore understandable that Twitmyer was always affectionately addressed as "Chief."

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Comparative Psychology Monographs—Baltimore, Md.; Williams & Wilkins Co. Subscription $6.00 per volume. 400 pages. Edited by Roy M. Dorcus. Without fixed dates, each number a single research. Founded 1922.

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